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Bonuses to Workers and Employers to Reduce Unemployment: Randomized Trials in Illinois

By STEPHEN A. WOODBURY AND ROBERT G. SPIEGELMAN*

New claimants for Unemployment Insurance were randomly assigned to one of two experiments that were designed to speed up the return to work. In the first experiment, a $500 bonus was offered to eligible claimants who obtained employment within 11 weeks. This experiment reduced the number of weeks of insured unemployment, averaged over all assigned claimants whether or not they participated, by more than one week. In the second experiment, the $500 bonus was offered to the subsequent employer of the eligible claimant. This experiment reduced the weeks of insured unemployment for only one important group — white women — by about one week.

Between mid-1984 and mid-1985, the Illinois Department of Employment Security conducted two controlled social experiments designed to test the effectiveness of cash bonuses in reducing the duration of insured unemployment. In one experiment, called the Claimant Bonus Experiment (or simply *Claimant Experiment*), a random sample of new claimants for Unemployment Insurance (UI) were instructed that they would qualify for a cash bonus of $500 if they found a job (of 30 hours or more per week) within 11 weeks of filing the UI claim, and if they held that job for four months. The intent was to create an incentive for claimants to search more intensely for work and to become reemployed more rapidly than they would otherwise.

In the other experiment, called the Employer Bonus Experiment (or *Employer Experiment*), a second random sample of new UI claimants were told that their *next employer* would qualify for a cash bonus of $500 if they, the claimants, found a job within eleven weeks of filing the UI claim, and if they retained that job for four months. The intent here was to provide a marginal wage-bill subsidy, or training subsidy, that might reduce the duration of insured unemployment.

The impetus for the Illinois Unemployment Insurance experiments was a decade of criticism of the UI system and the inability of economists to establish clearly how the behavior of UI recipients differs from what it would be in absence of the UI system, if it differs at all.

The most benign view of the UI system would be that it provides unemployment benefits to carry a worker through a spell of unemployment resulting from involuntary layoff. In such a benign view, UI benefits are taken to be a nondistortionary transfer the size of which cannot be affected by an individual's behavior. But there are at least two reasons why the presence of unemployment benefits may prolong a jobless spell beyond what it would be in the absence of unemployment benefits. First, as job search models such as those pioneered by Dale Mortensen (1970) and J. J. McCall (1970) suggest, UI benfits may act as a subsidy to additional job search. Indeed, nearly all empirical work to date on the relation between UI and the duration of unemployment has been interpreted in the context of one or another job search model. Second, as Orley Ashenfelter (1978a) has observed, UI benefits are also a subsidy to the consumption of nonmarket time (or leisure). If labor is not supplied perfectly inelastically (i.e., if workers place a positive value on their nonmarket time), then the availability of UI benefits may increase unemployment duration to the point where the marginal utility of nonmarket time equals the difference between the wage and the UI benefit level. Recently, Jerry Kingston et al. (1986) and Robert St. Louis et al. (1986) have suggested that many UI beneficiaries search for work less than the law requires, and that UI benefits subsidize leisure rather than job search.

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The Illinois experiments provide the first opportunity to explore, within a controlled experimental setting, whether bonuses paid to UI beneficiaries or their employers would reduce the unemployment of beneficiaries relative to a randomly selected control group.

The experiments yield strong evidence on whether the UI system is simply a benign income transfer that changes no worker or employer behavior. However, these experiments alone do not resolve the question of how best to characterize the behavior of unemployed workers. In Section I, we describe the design and operation of the experiments, and sketch our data sources. Section II discusses the incentives facing experimental participants. Section III is a presentation of the responses to the experimental treatments, and Section IV a development of experimental benefits and costs. Section V is a discussion of the implications of the experiments.

I. Experimental Design and Operations

A. Treatment Design

Both treatments consisted of a $500 bonus payment. In the Claimant Experiment, $500 was paid to a *claimant* who had found a job within 11 weeks and held that job for four months. In the Employer Experiment, $500 was paid to an *employer* who hired an eligible claimant within 11 weeks of the initial claim and employed that claimant for four months. The size of the bonus, $500, reflected a balancing of the experiments' budget constraint (a maximum of $750,000 in bonus payments) against a somewhat arbitrary judgment about how small a bonus could still be expected to generate a response. For the average UI claimant, $500 was on the order of 5 percent of annual wage and salary payments, and represented about four weeks of UI benefit payments. Such an amount was believed large enough to provide an incentive to at least some claimants to accept employment more quickly (or search more intensely), and to at least some employers to hire a claimant.[1]

To qualify for the $500 bonus (or to make their employer qualify), claimants had to find a job of 30 hours or more per week within 11 weeks of filing their initial claim, and to hold that job for at least four months. The period of 11 weeks was chosen arbitrarily to be about 40 percent of the potential duration of benefits in Illinois, which is 26 weeks. It was also chosen to be less than the median duration of insured unemployment experienced by Illinois UI beneficiaries in the months preceding the experiment.[2]

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[1] The $500 bonus was low compared with the marginal wage-bill subsidy provided under the Targeted Jobs Tax Credit, which was up to $3,000 in the first year after hiring an eligible worker. Because UI claimants would seem to be more attractive prospective employees than the disadvantaged workers who were eligible for the Targeted Jobs Tax Credit, it seemed likely that a smaller incentive might evoke a response from employers.

[2] Note that the 11-week period implies 10 weeks of benefit payments, because of the 1-week waiting period in Illinois.

Employment for four months was required to avoid the possibility of fraudulent hire, undertaken solely to obtain a bonus. Also, four months was regarded as the shortest period that would avoid payment of bonuses to seasonal workers and employers.

B. *Sample Design*

To be eligible for either the Claimant Experiment or the Employer Experiment, an individual had to 1) file an initial claim for UI between July 29, 1984 and November 17, 1984; 2) be eligible for 26 weeks of UI benefits; 3) register with one of 22 Job Service offices in northern and central Illinois; and 4) be at least 20 years old, but less than 55.

Imposing these eligibility criteria increased the homogeneity of the sample and excluded claimants whose behavior might be influenced by complicating factors. For example, enrolling claimants filing claims other than initial claims (i.e., additional, reopened, or transitional claims) would have meant admitting claimants eligible for anywhere from 1 to 26 weeks of UI benefits, and would have complicated the evaluation unnecessarily. Requiring enrollees to be Job Service registrants restricted the experiments to claimants who could be expected to obtain a job through usual market channels.[3] Excluding younger and older claimants was an attempt to reduce the number and kinds of complicating factors—special programs for young people, incentives to retire early for older workers—that might influence the job-finding behavior of those enrolled in the experiments.

Each claimant was assigned to one of three groups—the control group, the Claimant Experiment treatment group, or the Employer Experiment treatment group—by simple random assignment, based on the last two digits of his or her Social Security number. Hence, claimants had an equal probability of assignment to each of the three groups.

C. *Site Selection and Sample Size*

Three variables could have been used to control the size of the sample: the number (and type) of sites, the length of the enrollment period, and the proportion of claimants selected at any given site. Of the 22 Job Service offices chosen as sites, 11 were in metropolitan Chicago, 2 were in the outlying metropolitan area, and 9 were in outlying northern and central Illinois. Limiting the experiments to these 22 offices, rather than enrolling claimants at every Job Service office in the state, lowered the costs of monitoring the experiments and of communicating with Job Service personnel. On the other hand, having a fairly large number of sites helped assure that experimental results would not be specific to one or few offices, and that the results would represent the response of the diverse demographic and industrial mix of Illinois. Also, having more sites permitted a shorter enrollment period, which was viewed as desirable in order to obtain results in a reasonable period of time.

The duration of the experiments (originally designed to be 13 weeks, ultimately 16) was selected with an eye to 1) the size of the bonus budget, and 2) achieving a sample large enough to allow detection of experimental responses that would be relevant to policy. As it turns out, the sample was large enough to detect, at the 5-percent significance level, a 0.017 change in the proportion of claimants who found a job within 11 weeks (for example, a change from 25.6 to 27.3 percent). Such a small response is at the lower bound of what could be regarded as economically significant to UI reform (see our report, 1987a, ch. 2).

D. *Operational Issues and Data*

Although the experimental design was unusually simple, implementing it within an existing agency posed a variety of complexities (our report, 1987a, chs. 2 and 3). In particular, several special instruments had to be created to monitor and track the experience of enrolled claimants. When a claimant registered at the Job Service office, a base-line survey was administered to monetarily eligible claimants aged 20 through 54, and a special log was started on each claimant to record the claimant's treatment assignment and experience in the experiment. Claimants who were assigned to the Claimant Experiment or Employer Experiment groups were asked to sign an "agreement to participate," and those who agreed received a packet of administrative and instructional materials by mail once their nonmonetary eligibility for benefits had been determined. Participants in the Claimant Experiment who found a job within 11 weeks submitted a Notice of Hire (countersigned by the employer) to the Illinois Department of Employment Security (the Department), and those in the Employer Experiment who found a job within 11 weeks gave a Notice of Hire to their employer for submission to the Department. The Department then returned a voucher to the claimant or the employer, and the voucher was in turn submitted for $500 cash after the claimant had been employed continuously for four months. All of these transactions were recorded in the Job Service office logs.

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[3] Hence, workers on layoff with a definite recall date and union members who find jobs through a hiring hall were excluded from the design. In addition, recent veterans and federal employees were excluded.

To analyze treatment responses, we had access not only to the base-line survey and office logs, but also to the administrative data bases of the Department. Most important were 1) the Benefits Information System, which records the dates of claims filed and the amount and timing of benefits received, among other items, and 2) the Wage Records data base, from which we have drawn each claimant's quarterly earnings in UI-covered employment for the third quarter of 1983 through the third quarter of 1985 inclusive. Access to these administrative data frees us from the problem of selective attrition and associated nonresponse bias. That is, we have complete data on each claimant's experience in the experiment, on benefits received, and on earnings in covered employment before, during, and after the experiment.[4]

Table 1 displays basic data on experimental enrollment and use. Roughly 4,000 claimants who were eligible for UI benefits and eligible to participate in the experiments were assigned to each of the three experimental categories. Because the procedures used to construct these three samples were identical, each can be treated as a random sample from the population of fully eligible initial claimants for UI benefits who were aged 20 through 54.

The second row of Table 1 shows that there were important differences between the Claimant Experiment and Employer Experiment in claimants' willingness to participate in each. Whereas 84 percent of the eligible claimants who were offered the chance to participate in the Claimant Experiment agreed to participate, only 65 percent of those offered the chance to participate in the Employer Experiment agreed. Table 1 further indicates that actual use of the programs—as shown by return of Notices of Hire and actual cashing of bonuses—differed greatly between the Claimant Experiment and Employer Experiment. Whereas 14 percent of those assigned to the Claimant Experiment received a bonus, only 3 percent of those assigned to the Employer Experiment were responsible for a bonus payment to their employer.

We have explored participation in and use of the experiments in detail elsewhere (1987a, ch. 7), and for now note only two points about the Employer Experiment. First, the Employer Experiment was a more complicated treatment than the Claimant Experiment because it required the understanding and participation of both the worker and the employer. Second, the limited use of the Employer Experiment suggests that it had limited scope for reducing UI benefits paid or weeks of insured unemployment.

Table 2 displays some descriptive statistics of each of the three subsamples. Although these data on sex, race, and other variables may be of interest in their own right, their

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[4] The data do impose an important limitation: We do not know the labor force status of claimants who terminated their benefits (particularly those who exhausted benefits) but did not reenter UI-covered employment. These claimants could have found a job in the uncovered sector, left the labor force, or remained unemployed (i.e., continued to seek work).

TABLE 1—ILLINOIS UNEMPLOYMENT INSURANCE EXPERIMENTS: PROGRAM PARTICIPATION AND USE

	Control		Claimant Experiment		Employer Experiment	
	N	Proportion	*N*	Proportion	*N*	Proportion
Eligible[a,b]	3,952	1.00	4,186	1.00	3,963	1.00
Agreed to Participate[c]	–	–	3,527	0.84	2,586	0.65
Submitted Notice of Hire[d]	–	–	765	0.18	199	0.05
Bonus Paid	–	–	570	0.14	112	0.03

Sources: Eligibility from Illinois Department of Employment Security, Benefits Information System; other data from office logs kept during the experiments.

[a] Eligible for UI benefits by both monetary and nonmonetary criteria, met the age and initial claim restrictions of the experiments, and were located in the Benefits Information System.

[b] A total of 17,306 claimants completed the base-line survey and were assigned to one of the three groups; 1,857 of these were monetarily ineligible, were not initial claimants (that is, were filing additional, reopened, or transitional claims), or could not be located in the Benefits Information System. An additional 3,348 (1,171 controls, 1,104 in the Claimant Experiment, and 1,073 in the Employer Experiment) were nonmonetarily ineligible (i.e., failed to meet separation and availability requirements, as determined by our constructed nonmonetary eligibility code), or failed to meet the age restrictions of the experiments.

[c] Agreed to participate according to Job Service office records.

[d] Includes participants who ultimately received a bonus but never submitted a Notice of Hire.

TABLE 2—CHARACTERISTICS OF CLAIMANTS ASSIGNED TO EXPERIMENTAL GROUPS

	Control		Claimant Experiment		Employer Experiment	
	N	Proportion	*N*	Proportion	*N*	Proportion
Total	3,952	1.000	4,186	1.000	3,963	1.000
Male	2,162	0.547	2,357	0.563	2,131	0.538
White	2,497	0.632	2,723	0.651	2,565	0.647
Black	1,072	0.271	1,050	0.251	1,014	0.256
Hispanic, Native American, Other	383	0.097	413	0.099	384	0.097
Age 20–29	1,680	0.425	1,827	0.436	1,679	0.424
Age 30–39	1,315	0.333	1,357	0.324	1,292	0.326
Age 40–49	708	0.179	776	0.185	740	0.187
Age 50–54	248	0.063	226	0.054	252	0.064
Weekly Benefit Amount:						
$51	347	0.088	355	0.085	333	0.084
$52–$90	794	0.201	887	0.212	861	0.217
$91–$120	666	0.169	738	0.176	711	0.179
$121–$160	749	0.190	822	0.196	716	0.181
$161	1,396	0.353	1,384	0.331	1,342	0.339
Dependents' Allowance	1,834	0.323	1,955	0.345	1,883	0.332

Source: Illinois Department of Employment Security, Benefits Information System.

Notes: "Weekly Benefit Amount" refers to weekly payment for which each claimant was eligible at the time of filing the initial claim. The sample excludes claimants who were ineligible for UI benefits for monetary and nonmonetary reasons (as determined by our constructed nonmonetary eligibility code), and who failed to meet the initial claim and age restrictions of the experiments. Hence, all initial claimants who met the program criteria and were eligible for UI benefits are included in the sample.

main import lies in the support they lend to the randomness of the three subsamples. Indeed, none of the differences in proportions between any pair of groups is statistically different from zero at conventional significance levels.[5] The randomness of the three subsamples along the lines of observable characteristics suggests that each of the three subsamples was indeed randomly drawn from the same population. It follows that comparisons between the Claimant Experiment group and the control group (or between the Employer Experiment group and the control group) implicitly control for all observed and unobserved variables that may have contributed to the outcomes that are of interest—duration of insured unemployment and post-reemployment earnings. Thus, a simple comparison of the mean weeks of insured unemployment for members of either experimental group with the mean weeks of unemployment for members of the control group will show the impact of the treatment in question on the duration of insured unemployment.

II. Incentives Facing Participants

The incentives facing UI claimants assigned to the Claimant Experiment may be viewed within the context of either a job search model or an income-leisure model of labor supply. In a job search context, the $500 bonus creates an incentive for unemployed workers either to lower their reservation wage during the 11-week qualification period and accept a job sooner than they would otherwise, or to search more intensely for a job in order to find a job sooner than they would otherwise. In a labor supply context, the $500 bonus raises the opportunity cost of leisure consumed during the period of time immediately following the initial claim, hence creating an incentive to substitute income for leisure. The basic results of the Claimant Experiment cannot distinguish whether one of these approaches (or some other approach) is the appropriate way to view how the Claimant Experiment worked; such an enterprise requires imposition of a structural model on the data and we do not propose to do that here.

To the extent that leisure or job search can be substituted easily from one time period to another, some workers may alter their behavior both during the period of the experiment and during the period after reemployment. We shall test for this possibility in what follows by examining the behavior of the treatment group relative to the control group before, during, and after the experiment.

Marginal wage-bill subsidies resembling the Employer Experiment have been implemented in the United States before. As Ashenfelter (1978b) has observed, the evaluation of such subsidies is complicated by the fact that the employers who would add workers to their payrolls even without the subsidy have the greatest incentive to participate in such programs.[6] If an employer pays the same wage rate to all workers of a given skill class, then an employer who hires an Employer Experiment enrollee will simply receive a subsidy for hiring the enrollee rather than someone else. Clearly, then, a worker assigned to the Employer Experiment has an increased probability of being hired, but it does not follow that the Employer Experiment must increase total employment. The higher the turnover rate of workers in a labor market, given the ratio of Employer Experiment enrollees to total job seekers in that market, the less likely it is that the Employer Experiment will raise total employment. Nevertheless, if the demand for labor is sufficiently elastic, the Employer Experiment bonus may induce an employer who was not planning to hire additional workers to increase total employment.[7]

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[5] We have explored differences between the control group and the experimental groups in several other observable variables, and have found no statistically significant differences.

[6] There is of course a similar possibility in the Claimant Experiment. Some workers assigned to the Claimant Experiment—i.e., those whose planned unemployment durations were shorter than 11 weeks—could receive a lump sum bonus without altering their behavior at all. Nevertheless, if there is uncertainty about whether one can find a job to fulfill precisely one's plan, even these workers (if they are risk averters) may be induced by the $500 bonus to become reemployed sooner than they otherwise would.

A final consideration is that a worker assigned to the Employer Experiment may bargain with an employer to receive a wage payment that is (in expected present value terms) higher than would otherwise be the case by precisely the $500 bonus that the employer will receive. If workers could strike such bargains, which could be thought of as "Coasean deals," then the Employer Experiment and the Claimant Experiment would establish precisely the same incentives for workers, and the two experimental treatments should have precisely the same effect on workers' unemployment and earnings (net of the bonus).[8]

III. Responses to the Treatments

A. *Effects on Benefit Receipt and Duration of Insured Unemployment*

Table 3 displays the means of several program variables by experimental group. These means are based on the sample of all fully eligible claimants—the same sample that underlies Tables 1 and 2. We stress that this sample *includes* eligible claimants who refused to participate in one of the experiments, so that the Claimant Experiment and Employer Experiment groups are fully comparable with the control group. (Examining only Claimant Experiment and Employer Experiment group members who agreed to participate would involve a comparison of self-selected groups—Claimant Experiment and Employer Experiment *agreers*—with all controls, some of whom would have refused participation had they been offered the opportunity. The result could be biased estimates of the experimental effects.)

Row 1 under "Benefits Paid" ("State Regular, First Spell") shows that the mean dollar amount of state regular benefits received by members of the control group during the spell of unemployment immediately following the initial claim was $2,267. For eligible claimants assigned to the Claimant Experiment and Employer Experiment, the comparable figures are $2,074 and $2,159. Row 2 under "Benefits Paid" shows the mean of the sum of state regular benefits and Federal Supplemental Compensation received during the spell of unemployment immediately following the initial claim. Rows 3 and 4 show state regular benefits received, and the sum of state regular and Federal Supplemental Compensation received, but this time for the entire benefit year.

The full benefit year is the appropriate time period to examine in determining the impact of the experiments on benefit receipt, rather than just the spell of insured unemployment following the initial claim. As noted in Section II, it is possible that the experimental treatments created incentives to redistribute insured unemployment over the benefit year, with insured unemployment dropping immediately following the initial claim, but increasing in the latter part of the benefit year to compensate. We can capture this effect, if it exists, by examining benefit receipt and weeks of insured unemployment over the full benefit year.

The two rows under "Weeks of Insured Unemployment" show the means—for the spell of unemployment immediately following the initial claim, and for the full benefit year—of the number of weeks of insured unemployment for each group. It is worth emphasizing that statistical tests performed on these means are valid if interpreted as tests on the number of weeks of *insured* unemployment, because weeks of insured unemployment are a censored measure of total unemployment.

The entries at the bottom of Table 3 show the proportion of claimants who exhausted their state regular benefits, and the proportion of claimants who terminated benefits within 11 weeks of filing their initial claim.

Table 4 displays differences between the mean values of the control group and the

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[7] For example, John Bishop (1987, ch. 4) has recently concluded that the Targeted Jobs Tax Credit did result in a net increase in employment in participating firms.

[8] See Ronald Coase (1960). Of course, it is also possible for the Claimant Experiment and the Employer Experiment to have identical effects without such Coasean deals occurring.

TABLE 3—MEANS OF PROGRAM VARIABLES BY EXPERIMENTAL GROUP[a]

	Control		Claimant Experiment		Employer Experiment	
	Mean	*SE* of Mean	Mean	*SE* of Mean	Mean	*SE* of Mean
Benefits Paid ($):						
1) State Regular, First Spell	2,267	27.5	2,074	26.7	2,159	27.4
2) Total, First Spell	2,558	33.8	2,329	32.9	2,446	33.8
3) State Regular, Benefit Year	2,487	27.0	2,328	26.3	2,426	27.0
4) Total, Benefit Year	2,786	33.1	2,592	32.2	2,725	33.8
Weeks of Insured Unemployment:						
1) First Spell	18.3	0.205	17.0	0.199	17.7	0.205
2) Benefit Year	20.1	0.194	18.9	0.188	19.7	0.194
	Proportion	*SE* of Proportion	Proportion	*SE* of Proportion	Proportion	*SE* of Proportion
Proportion of Claimants Who:						
1) Exhausted Benefits	0.478	0.008	0.446	0.008	0.464	0.008
2) Ended Benefits within 11 weeks	0.353	0.008	0.408	0.008	0.384	0.008
N	3,952		4,186		3,963	

Sources: Tabulations from Illinois Department of Employment Security, Benefits Information System, and office logs.

Notes: "First Spell" refers to the spell of unemployment immediately following the initial claim for UI. "Total Benefits Paid" refers to the sum of state regular benefits and Federal Supplemental Compensation. "Benefit Year" refers to benefits paid or weeks of benefits paid during the full benefit year for each claimant. "Ended Benefits within 11 Weeks" refers to termination of benefits within 11 weeks of filing the initial claim (equivalently, 10 or fewer weeks of benefit payments, because of the 1-week waiting period). Sample is the same as that underlying Table 2.

[a] *SE* denotes standard error.

Claimant Experiment group, and between the control group and the Employer Experiment group. The differences are calculated from Table 3, and the standard error of each difference is shown. The most striking results shown by the table pertain to the Claimant Experiment: Average benefit receipt was lower in the Claimant Experiment group than in the control group by $158 to $194 over the full benefit year (depending on whether Federal Supplemental Compensation benefits are included in benefits received). The differences are statistically different from zero at the 1-percent level (using a two-tailed test). Further, the average number of weeks of insured unemployment was lower in the Claimant Experiment group than in the control group by somewhat over a week, again measured over the full benefit year.

The Claimant Experiment results are quite strong in that the $158 to $194 benefit reduction, and the 1.15-week reduction in the duration of unemployment, were attained on average over all eligible workers who were assigned to the Claimant Experiment, whether or not they agreed to participate, and whether or not they actually cashed a voucher for $500. Further, compared with the control group, 5.5 percent more of those assigned to the Claimant Experiment ended their spell of insured unemployment within 11 weeks of filing, and 3.2 percent fewer exhausted their UI benefits. These last findings suggest that the Claimant Experiment

TABLE 4—DIFFERENCES BETWEEN CONTROL GROUP AND EXPERIMENTAL GROUP MEANS

	Claimant Experiment minus Control		Employer Experiment minus Control	
	Difference of Means	*SE*	Difference of Means	*SE*
Benefits Paid ($):				
1) State Regular, First Spell	−193[a]	38.30	−108[a]	38.82
2) Total, First Spell	−229[a]	47.15	−112[b]	47.79
3) State Regular, Benefit Year	−158[a]	37.70	−61	38.21
4) Total, Benefit Year	−194[a]	47.17	−61	46.80
Weeks of Insured Unemployment:				
1) First Spell	−1.37[a]	0.29	−0.67[b]	0.29
2) Benefit Year	−1.15[a]	0.29	−0.36	0.27
	Difference of Proportions	*SE*	Difference of Proportions	*SE*
Proportion of Claimants Who:				
1) Exhausted Benefits	−0.032[a]	0.011	−0.014	0.011
2) Ended Benefits within 11 Weeks	+0.055[a]	0.011	+0.031[a]	0.011

Source: Calculations based on Table 3. See Notes and fn. a, Table 3.

[a] Rejection of the hypothesis that the difference of means is zero using a two-tailed 1-percent significance level.

[b] Rejection of the hypothesis that the difference of means is zero using a two-tailed 5-percent significance level.

reduced the duration of insured unemployment of workers throughout the distribution of unemployment spell lengths.

The results of the Employer Experiment are quite different. Although there was an initial reduction in benefits received by the Employer Experiment group in the spell of unemployment immediately following the initial claim, the reduction in benefits paid to the Employer Experiment group over the full benefit year is statistically insignificantly different from zero. The evidence of an impact of the Employer Experiment on the number of weeks of insured unemployment is similar. Although there was a reduction in unemployment during the first spell, no statistically significant difference between the control group and the Employer Experiment group exists over the full benefit year.

In view of the comparatively low rate of use of the Employer Experiment, it may seem surprising that there was an impact even in the first spell of unemployment. The results suggest that, to the extent the Employer Experiment reduced the length of the initial spell of unemployment, this effect did not persist over the full benefit year. However, evidence presented in Section III, Part C, shows that the Employer Experiment did reduce the benefits paid and the weeks of insured unemployment of white women over the full benefit year. Hence, these overall results mask an effect of the Employer Experiment on at least one major group of UI claimants.

B. *Effects on Earnings after Reemployment*

The above results suggest strongly that the Claimant Experiment reduced the duration of job search for claimants who participated in it. It is possible—indeed, Mortensen's and McCall's early models of job search suggest

TABLE 5—MEAN PRE- AND POSTPROGRAM EARNINGS OF ELIGIBLE CLAIMANTS WITH EARNINGS IN QUARTER AFTER BENEFIT TERMINATION, BY EXPERIMENTAL GROUP

Mean Earnings in:	Control	Claimant Experiment	Employer Experiment
Base Period (Average of Four Quarters)	\$3,226 (44.7) ($N = 2{,}531$)	\$3,243 (44.4) ($N = 2{,}786$)	\$3,203 (45.3) ($N = 2{,}550$)
Quarter before Initial Claim	3,995 (53.1) ($N = 2{,}357$)	3,965 (48.0) ($N = 2{,}591$)	3,916 (50.0) ($N = 2{,}386$)
Quarter after Benefit Termination	3,121 (47.3) ($N = 2{,}531$)	3,129 (46.7) ($N = 2{,}786$)	3,066 (46.7) ($N = 2{,}550$)

Notes: Standard error of each mean is shown in parentheses. The sample is constructed as follows: Starting with fully eligible claimants who met the initial claim and age restrictions of the experiments, samples of those who showed positive earnings in the quarter after they terminated benefits (2,531 controls, 2,786 Claimant Experiment enrollees, and 2,550 Employer Experiment enrollees) were used to compute mean earnings in the base period, the quarter before the initial claim, and the quarter after benefit termination. Note that all means are computed conditional on positive earnings; thus, N used to compute mean earnings in the quarter before the initial claim is lower than elsewhere because not all claimants in the sample showed earnings in the preclaim quarter.

—that the shorter search time induced by the \$500 bonus may result in a less-favorable match between worker and job, which would manifest itself in lower earnings in the subsequent job. If a Claimant Experiment participant who submitted a Notice of Hire (or received a bonus) effectively lowered his or her reservation wage and simply accepted the first job that presented itself, then the claimant's earnings after reemployment and the efficiency of the labor market would both be reduced.[9]

Table 5 addresses the concern that Claimant Experiment participants may have sacrificed earnings in their postprogram job in order to obtain the \$500 bonus. The table displays data on the pre- and postprogram earnings of claimants in each of the three groups. All figures are based on the subsample of claimants who terminated benefits (at some point following the initial claim that brought them into the experiment), and had positive earnings in the first full quarter following benefit termination. That is, claimants who exhausted benefits and failed to find a new job, and claimants who dropped out of the labor force, are excluded from consideration here. Since our concern focuses on the earnings of those who found a new job, and whether these earnings are lower for the Claimant Experiment group, this is clearly the appropriate group to examine.

The first row of Table 5 shows average base period earnings of claimants in each of the three groups, and the second row shows earnings in the quarter before the initial claim was filed. Note that there is no statistically (or otherwise) significant difference across groups in either of these preprogram earnings measures.[10] (The sample on which earnings in the quarter before the initial claim is based is smaller than the sample used to calculate the other figures in the table because not all claimants had earnings in the quarter before they filed for benefits.)

The third row of Table 5 shows, for each of the three groups, earnings in the first full quarter after benefit termination (for those claimants who had earnings after benefit termination). The figures suggest strongly

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[9] Kathleen Classen (1979) has shown that, in search models that relax some of the assumptions of the early Mortensen and McCall models, the relation between unemployment duration and earnings after reemployment is ambiguous. See also Kenneth Burdett (1979).

[10] For mean earnings in the base period, the standard error of the difference between either experimental group and the control group is about \$63, which overwhelms either of the mean differences (\$17 and \$23). The standard error of the differences is about \$72 for mean earnings in the quarter before the initial claim, and about \$67 for mean earnings in the quarter after benefit termination.

TABLE 6—DIFFERENCE BETWEEN CONTROL GROUP AND EXPERIMENTAL GROUP MEANS BY RACE AND SEX

	Difference in Benefits Paid ($): Control vs.		Difference in Weeks of Benefits: Control vs.	
Race/Sex	Claimant Experiment	Employer Experiment	Claimant Experiment	Employer Experiment
White Women	−262.0[a]	−164.1[b]	−1.623[a]	−1.008[b]
(1,113; 1,170; 1,166)	(70.7)	(70.7)	(0.503)	(0.503)
White Men	−185.1[a]	−9.8	−1.125[b]	+0.192
(1,384; 1,553; 1,399)	(62.4)	(63.6)	(0.444)	(0.455)
Black Women	−65.9	−49.3	−0.978	−0.539
(527; 512; 504)	(104.7)	(105.1)	(0.745)	(0.748)
Black Men	−72.7	−33.2	−0.518	−0.211
(545; 538; 510)	(115.6)	(104.3)	(0.730)	(0.742)

Notes: A negative difference implies that the experimental mean is less than the control mean. Standard error of each difference is in parentheses under the difference. Number of observations in the control, Claimant Experiment, and Employer Experiment groups for each race and sex category is shown in parentheses in the first column. Sample sizes for Hispanics, Native Americans, and others are too small to allow detection of experimental effects of the magnitude found in the overall sample; hence, these groups are not considered separately.

[a,b]See Table 4.

that there is no difference between the post-program earnings of controls and of Claimant Experiment workers—the average for the controls is $3,121, whereas the average for the Claimant Experiment group is $3,129. The difference, $8, is swamped by the standard error of that difference, which is $67. We conclude that the relatively rapid reemployment of Claimant Experiment participants did *not* come at the expense of lower earnings. Rather, the data are consistent with the idea that the faster reemployment of Claimant Experiment workers resulted from more-intense job search efforts by Claimant Experiment workers, and not from overly rapid acceptance of job offers.[11]

C. *Effects by Race and Sex*

Those enrolled in the experiments compose a diverse group, and it is possible that certain groups of workers responded more strongly than others to the experimental incentives. We have explored elsewhere (1987a, ch. 6) the effects of the experiments on several different categories of workers and we believe one of these breakdowns to be particularly significant.

Table 6 displays the effects of the experiments on 1) state regular benefits paid to claimants during their full benefit year, and 2) the number of weeks of benefits paid to claimants during the full benefit year, broken down by four race and sex categories.[12] These results are dramatic because they show that, for white women, the Employer Experiment unambiguously reduced UI benefit payments and weeks of insured unemployment. Moreover, we have found that no loss of earnings accompanied the response of white women and their employers to the Employer Experiment (1987a, ch. 6, Table 6–9). White women stand out sharply from the other three race and sex categories, each of whom showed a response to the Employer Experiment that is not statistically significantly different from zero.

Table 6 also shows that the effects of the Claimant Experiment appear to have varied by race. The Claimant Experiment brought about a larger reduction in the UI benefits and weeks of insured unemployment of both white women and white men than of black women and black men.[13] Indeed, the effect of the Claimant Experiment on the insured unemployment of blacks is not statistically significantly different from zero.

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[11] An interpretation within the framework of labor supply is also possible. If a single wage offer (constant over time) faced a worker, then increased search intensity would be unnecessary in order to obtain a job at a given wage. Participating claimants could then cut short their spell of unemployment by substituting income for leisure in the current period.

[12] Race and sex subgroups other than the four shown are too small to allow detection of experimental effects of the magnitude found in the overall sample.

Some insight into the differences by race and sex in the effects of the experiments can be obtained from Table 7. The first panel shows the proportion of each race and sex group in each experiment that received a bonus. Here we see that whites were nearly three times more likely to receive a Claimant Experiment bonus than were blacks. It is also shown that whites' employers were roughly ten times more likely to receive an Employer Experiment bonus than were blacks' employers.

We can decompose the difference between any two groups in their propensities to receive a bonus into two components. One component reflects the difference between the groups in their probabilities of qualifying for a bonus (or in their probabilities of qualifying an employer for a bonus), and the other reflects the difference in the probabilities that qualifying workers (or their employers) will actually cash a voucher for $500. Define b_{it} as the proportion of race and sex group i (i = white women, white men, black women, and black men) in treatment group t (t = control, Claimant Experiment, and Employer Experiment) who actually receive a bonus. Also, define e_{it} as the proportion of all claimants in group i, t who qualify for a bonus (because they find a job within eleven weeks and hold it for four months), and r_{it} as the proportion of *qualifying* claimants in group i, t who actually receive a bonus (or whose employers do). (We will call this conditional probability r_{it} the "take-up rate" because it reflects the proportion of qualifying claimants who use the program.) We can then express the proportion of a group who receive a bonus (b_{it}) as the product of the proportion qualifying and the take-up rate: $b_{it} = e_{it} r_{it}$.

Since we are interested in differences between groups in their propensities to receive a bonus, it is natural to write the proportional difference between two groups as

$$(1) \qquad b_{it}/b_{js} = (e_{it} r_{it})/(e_{js} r_{js}).$$

Taking the natural logarithm of both sides and rearranging gives

$$(2) \quad \ln(b_{it}/b_{js}) = \ln(e_{it}/e_{js}) + \ln(r_{it}/r_{js}).$$

This is the desired decomposition. It attributes differences between two groups' propensities to receive a bonus to differences between the two groups in (a) their probabilities of qualifying for a bonus, and (b) their probabilities of cashing a voucher if they qualify.

The data required to compute any such decomposition are displayed in Table 7. The second panel shows the proportion of each race and sex group in each experimental group that (by finding a job within 11 weeks and holding the job for four months) qualified for a bonus. The third panel shows the take-up rate of each group. If we were interested in decomposing the difference between white and black women's probabilities of receiving a Claimant Experiment bonus, we would compute:

$$\ln(b_{wf,a}/b_{bf,a}) = \ln(0.1761/0.0625) = 1.036;$$

$$\ln(e_{wf,a}/e_{bf,a}) = \ln(0.300/0.158) = 0.641;$$

$$\ln(r_{wf,a}/r_{bf,a}) = \ln(0.5870/0.3951) = 0.396.$$

(Subscripts *wf* and *bf* denote white and black females, and subscript *a* denotes the Claimant Experiment.) The decomposition suggests that 62 percent of the difference between white and black women in their propensities to receive Claimant Experiment bonuses can be attributed to the higher probability that white women qualified for a bonus, and that 38 percent of the difference can be attributed

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[13] A case could be made that black women showed a stronger response to the Claimant Experiment treatment than did black men.

TABLE 7—PROPORTION OF CLAIMANTS WHO RECEIVED A BONUS, QUALIFIED FOR A BONUS, TAKE-UP RATE, AND EXPERIMENTAL COSTS AND BENEFITS, BY RACE AND SEX

	Control	Claimant Experiment	Employer Experiment
1) Proportion Receiving a Bonus:			
Total	–	0.1362	0.0283
		(0.0053)	(0.0026)
White Women	–	0.1761	0.0463
		(0.0111)	(0.0062)
White Men	–	0.1700	0.0358
		(0.0095)	(0.0050)
Black Women	–	0.0625	0.0040
		(0.0107)	(0.0030)
Black Men	–	0.0632	0.0039
		(0.0105)	(0.0028)
2) Proportion Qualifying for a Bonus:			
Total	0.207	0.250[a]	0.228[a]
	(0.006)	(0.007)	(0.007)
White Women	0.237	0.300[a]	0.280[a]
	(0.013)	(0.013)	(0.013)
White Men	0.244	0.277[a]	0.258
	(0.012)	(0.011)	(0.012)
Black Women	0.139	0.158	0.153
	(0.015)	(0.016)	(0.016)
Black Men	0.127	0.165	0.120
	(0.014)	(0.016)	(0.014)
3) Take-up Rate:			
Total	–	0.5448	0.1241
		(0.0077)	(0.0052)
White Women	–	0.5870	0.1654
		(0.0263)	(0.0109)
White Men	–	0.6139	0.1388
		(0.0235)	(0.0092)
Black Women	–	0.3951	0.0261
		(0.0543)	(0.0071)
Black Men	–	0.3821	0.0325
		(0.0515)	(0.0079)
4) Bonus Cost per Assigned Claimant ($):			
Total	–	68.1	14.2
		(2.7)	(1.3)
White Women	–	88.1	23.2
		(5.6)	(3.1)
White Men	–	85.0	17.9
		(4.8)	(2.5)
Black Women	–	31.3	2.0
		(5.4)	(1.5)
Black Men	–	31.6	2.0
		(5.3)	(1.4)
5) Ratio of Benefit Payment Reduction to Bonus Cost per Assigned Claimant:			
Total	–	2.32	4.29
		(0.46)	(2.90)
White Women	–	2.97	7.07
		(0.61)	(2.95)
White Men	–	2.18	0.55
		(0.61)	(3.48)
Black Women	–	2.11	24.65
		(2.98)	(34.06)
Black Men	–	2.30	16.60
		(3.27)	(40.50)
6) Size of Group (N):			
Total[b]	3,952	4,186	3,963
White Women	1,113	1,170	1,166
White Men	1,384	1,553	1,399
Black Women	527	512	504
Black Men	545	538	510

Note: Standard errors in parentheses. The proportion of each group qualifying for a bonus (second panel) is the proportion of the group that found a job within 11 weeks of filing the initial claim *and* held the job for four months. A group's take-up rate (third panel) is the proportion of the group receiving a bonus divided by the proportion qualifying for a bonus. A group's bonus cost per assigned claimant (fourth panel) equals total bonus payments to the group divided by the size of the group (N). The ratio of benefit payment reduction to bonus cost per assigned claimant (fifth panel) states the reduction in UI benefit payments for each dollar of bonus paid. The standard error of each ratio in the fifth panel is approximated by taking a Taylor expansion for the ratio.

[a] Rejection of the hypothesis that the difference of proportions between the experimental and control groups is zero using a two-tailed 5-percent significance level.

[b] The sum of white women, white men, black women, and black men is less than the total because the total includes Hispanics, Native Americans, and others.

TABLE 8—DECOMPOSITION OF DIFFERENCES IN PROBABILITIES OF BONUS RECEIPT, SELECTED GROUP COMPARISONS

		Percentage of Difference Attributable to:	
Comparison	Difference in Proportions Receiving Bonus	Difference in Proportions Qualifying	Difference in Take-up Rates
Women in Claimant Experiment with Men in Claimant Experiment	0.0009 (0.0100)	[a]	[a]
Whites in Claimant Experiment with Blacks in Claimant Experiment	0.0979 (0.0112)	58	42
Women in Employer Experiment with Men in Employer Experiment	0.0073 (0.0100)	[a]	[a]
Whites in Employer Experiment with Blacks in Employer Experiment	0.0327 (0.0113)	29	71
Women in Claimant Experiment with Women in Employer Experiment	0.1045 (0.0073)	3	97
Men in Claimant Experiment with Men in Employer Experiment	0.1109 (0.0067)	8	92
Whites in Claimant Experiment with Whites in Employer Experiment	0.1241 (0.0057)	5	95
Blacks in Claimant Experiment with Blacks in Employer Experiment	0.0590 (0.0098)	6	94

Note: Standard error of the difference in proportions receiving a bonus is shown in parentheses below the difference. Equation (2) in the text defines the decomposition displayed in the table.

[a] The difference in proportions receiving a bonus is statistically insignificant; therefore, no decomposition is shown.

to the higher probability that qualifying white women actually cashed a $500 voucher.

Some further decompositions are shown in Table 8. To keep Table 8 of reasonable length only four groups—all women, all men, all whites, and all blacks—are used in the comparisons (rather than the less aggregate race and sex categories shown in Table 7). The top four rows of Table 8 display comparisons of different groups within each experiment, whereas the bottom four rows display comparisons of the same group between experiments.

The differences between women and men in bonus receipt within each experiment are statistically insignificant and hence no decompositions are shown (first and third rows of the table). But the differences between whites and blacks within experiment are more revealing (second and fourth rows). These decompositions show that differences between whites and blacks in bonus receipt can be attributed to white-black differences both in the probabilities of qualifying for a bonus and in take-up rates. In the Claimant Experiment, 58 percent of the higher propensity of whites to receive a bonus can be attributed to whites' higher probability of qualifying, and 42 percent to whites' higher take-up rate. In the Employer Experiment, only 29 percent of the higher propensity of whites to receive a bonus can be attributed to whites' higher probability of qualifying, and 71 percent to the higher take-up rate of whites and their employers.

Differences between the Claimant Experiment and the Employer Experiment in bonus receipt are decomposed in the bottom four rows of Table 8. The decompositions show clearly that the difference between the experiments in bonus receipt must be attributed mainly to higher take-up rates in the Claimant Experiment.

IV. Benefit-Cost Ratios and Take-up Rates

The fourth and fifth panels of Table 7 display information on the costs and benefits of the experiments to the state. The fourth panel shows the bonus cost per assigned claimant for each experimental and race-sex group. For example, the figure $68.10 for bonus cost per assigned claimant in the Claimant Experiment equals the dollar amount paid in Claimant Experiment bonuses ($500 times 570 bonuses) divided by the number of claimants assigned to the experiment (4,186). Since the Claimant Experiment was more heavily used than the Employer Experiment, its costs per assigned claimant were higher.

The cost figures displayed in the fourth panel of Table 7 can be used to derive a benefit-cost ratio for each experimental and race-sex group. These ratios are shown in the fifth panel. They are obtained by dividing the reduction in UI benefit payments per claimant (that is, the average treatment responses shown in Tables 4 and 6) by the bonus cost per assigned claimant from the fourth panel of Table 7. For example, the benefit-cost ratio for the Claimant Experiment, 2.32, equals the average treatment response ($158) divided by the bonus cost per assigned claimant ($68). This benefit-cost ratio is statistically significantly different from zero. A straightforward interpretation of this benefit-cost ratio is that, for the Claimant Experiment overall, state regular benefit payments were reduced by $2.32 for each $1.00 of bonus payments made. It follows that a program modeled on the Claimant Experiment would be extremely attractive from the state's point of view if the presence of the program did not increase unemployment among workers who were not participants in the program.

The fifth panel also shows that the overall benefit-cost ratio for the Employer Experiment is 4.29, but it is not statistically different from zero. The benefit-cost ratio for white women in the Employer Experiment, however, is 7.07, and is statistically different from zero. Hence, a program modeled on the Employer Experiment also might be attractive from the state's point of view if the program did not increase unemployment among nonparticipants. Since, however, the Employer Experiment affected only white women, it would be essential to understand the reasons for the uneven effects of the treatment on different groups of workers before drawing conclusions about the efficacy of such a program.

It is important to consider these benefit-cost ratios in relation to the take-up rates developed in the previous section and displayed in the third panel of Table 7. Those figures show that only 54 percent of the Claimant Experiment workers who qualified for a $500 bonus actually took the steps to claim the bonus. It is also striking that only 12 percent of the employers who could have received a $500 bonus simply by claiming it actually did so.

If a program modeled on either the Claimant or Employer Experiment were implemented, it is possible that claimants' or employers' take-up rates would be substantially higher than those observed in the experiments. Futher, if increased take-up rates were unaccompanied by additional reductions in UI benefits and unemployment, then the benefit-cost ratio would decline. For example, in the Claimant Experiment, if 100 percent of the qualifying claimants had claimed a bonus, then the benefit-cost ratio would fall from 2.32 to 1.26. In the Employer Experiment, if 100 percent of qualifying employers had claimed a bonus, then the benefit-cost ratio would be only 0.53. (For employers of white women in the Employer Experiment, a 100 percent take-up rate would lead to a decrease in the benefit-cost ratio from 4.29 to 1.17.) Again, these calculations assume an increase in the take-up rate without any accompanying behavioral response. How take-up rates would change in an actual program, and whether there would be further reductions in UI benefit payments and unemployment, are important topics for further research and experimentation.

V. Conclusion

The results of the Claimant Experiment are unequivocal and strong. The incentive created by the $500 bonus, which was actually paid to only 570 out of 4,186 UI claimants assigned to the experiment, reduced state regular benefits paid to the randomly selected treatment group by an average of $158, and reduced average weeks of insured unemployment by more than one week (over the full benefit year), compared with the randomly selected control group. We reemphasize that these reductions in average benefit payments and weeks of unemployment were achieved over all 4,186 eligible claimants in the Claimant Experiment sample, whether or not they agreed to participate or acted on the incentive.

Some of the results of the Employer Experiment are also unequivocal and strong. White women who were randomly assigned to the Employer Experiment received $164 less in UI benefits, and experienced one week less of insured employment (over the full benefit year), than did white women who were randomly assigned to the control group. These reductions were achieved on average over all 1,166 white women who were assigned to the Employer Experiment. Only 54 of these women were responsible for a $500 bonus being received by the employer who hired them. White women are the only race and sex group who experienced a statistically significant effect of the Employer Experiment.

It is clear that the members of the experimental treatment groups experienced less unemployment than the randomly selected control group. Thus, we may unambiguously reject the hypothesis that the unemployment insurance benefit system is only a benign or nondistortionary income transfer. The results reported here are the first experimental demonstration of this proposition, and we believe them to be quite convincing.

It is also clear that the members of the experimental groups were, on average, made no worse off by their assignment to the experiments. On the other hand, it is not possible to be certain whether the control group's experience was identical to what it would have been in the absence of any experimental treatments. This is a problem shared by all controlled social experiments. Moreover, we have not addressed whether a full-scale program modeled on either the Claimant or Employer Experiment would have significant displacement effects—that is, would result in improvements for program participants at the expense of nonparticipants. Hence, we cannot conclude unambiguously what the net social benefits of such a program might be.

Another limitation of our results stems from our finding that many claimants, and most employers failed to take the steps to obtain a $500 bonus even when they qualified to do so. If the take-up rate in the Claimant Experiment had been 100 percent (that is, if all claimants who qualified for a bonus had claimed one), and UI benefit reductions had remained unchanged, then the benefit-cost ratio of the experiment would have fallen to 1.26 from 2.32. If the take-up rate in the Employer Experiment had been 100 percent, then the benefit-cost ratio would have fallen to 0.53 from 4.29. (For white women in the Employer Experiment, the benefit-cost ratio would have fallen to 1.17 from 4.29.) Thus, even with a 100 percent take-up rate, the Claimant Experiment would have reduced UI benefit costs by considerably more than the bonus costs incurred; however, the Employer Experiment (overall) would not. Because the take-up rates observed in the experiments could understate those that would occur in a program modeled on one of the experiments, these findings pose important questions for further research and experimentation: Would take-up rates change in an actual program, and if so, how? To what degree would changes in take-up rates be accompanied by further reductions in UI benefit payments and unemployment?

Our analysis of the experimental data does not allow us to determine conclusively why the experimental treatments changed the participants' behavior. We do have three clear findings, however, that may be of considerable significance for evaluating alternative models of worker behavior: 1) Offering unemployed workers the option of assigning bonuses to employers who hired them had a far smaller effect on employment than offering unemployed workers the same bonus directly. 2) The lower unemployment of members of the Claimant Experiment group occurred for workers at all parts of the distribution of unemployment spell lengths. 3) The earnings of members of the treatment groups after they became reemployed did not differ from the earnings of members of the control group. It seems clear that additional analysis of these data will be needed before further conclusions can be reached, particularly conclusions about which of various models of worker and firm behavior are supported.

Because such strong inferences can be derived from experimental work, the desirability of further experimentation seems unambiguous. A significant improvement in the design of the Illinois experiments over the design of the income maintenance experiments of the 1960's and 1970's is the reliance on administrative records rather than surveys for the collection of information about the treatment and control groups.[14] Use of administrative records is an important development in field experimentation in economics. Experiments that rely solely on survey records are vastly more expensive than those that rely on administrative data. Moreover, experiments that rely on survey data to measure responses run the risk of confounding measures of the experimental treatment with measures of attrition from the survey, as Ashenfelter and Mark Plant (1987) have recently noted.

Although the Claimant and Employer Experiments in Illinois have yielded strong results about the effects of experimental bonuses to workers and employers, additional research and experimentation along the lines of these experiments could usefully address three different and important issues. First, it would be desirable to measure the response to variation in (a) the size of the bonus provided to successful job seekers and to employers, and (b) the length of the period within which a UI claimant must become reemployed in order to qualify for a bonus. Second, it is important to learn how a longer and more widespread experimental bonus program would influence employee and employer take-up rates as well as UI benefit reductions. Third, analysis and experimentation that would shed light on the extent to which differences between treatment and control groups in unemployment experience represent costs to the control group (or other nonparticipants) would be of special importance. Further experiments hold out the possibility of designing new programs that may effectively reduce unemployment at low or even negligible cost to unemployed workers and to society.

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Self-Selection and the Earnings of Immigrants

By GEORGE J. BORJAS*

This paper analyzes the way in which the earnings of the immigrant population may be expected to differ from the earnings of the native population because of the endogeneity of the decision to migrate. The empirical study shows that differences in the U.S. earnings of immigrants with the same measured skills, but from different home countries, are attributable to variations in political and economic conditions in the countries of origin at the time of migration.

Immigrants in the United States do not make up a random sample of the population from the countries of origin. This is perhaps the most convincing finding in the literature that analyzes how immigrants perform in the U.S. labor market. In the "first-generation" studies of this literature (Barry Chiswick, 1978; Geoffrey Carliner, 1980; Gregory DeFreitas, 1980), cross-section earnings functions were estimated and two conclusions were reached: 1) the age-earnings profile of immigrants is steeper than the age-earnings profile of the native population with the same measured skills; and 2) the age-earnings profile of immigrants crosses the age-earnings profile of natives about ten to fifteen years after immigration. Thus, after a relatively short adaptation period, immigrant earnings "overtake" the earnings of comparable native workers. The first of these findings was often explained in terms of the human capital framework: Immigrants presumably have stronger investment incentives than native workers, and hence immigrant earnings grow at a faster rate than native earnings. The existence of the overtaking age, however, was explained in terms of the unobserved characteristics of the migrants: Immigrants are a self-selected group and, as a result, immigrants may be "more able and more highly motivated" (Chiswick, p. 900) than the native born.

Recently, the focus has shifted from analyses of single cross-section data sets to studies of cohort or longitudinal data (see my 1985 and 1987 papers; and Guillermina Jasso and Mark Rosenzweig, 1985, 1986). The departure point for these studies is the well-known fact that the analysis of a single cross section of data cannot separately identify aging and cohort effects.[1] The cross-section finding that immigrant earnings and years since migration are positively correlated can be explained either in terms of an aging effect (i.e., assimilation) or it may be due to cohort differences in quality (caused by nonrandom return migration propensities and/or secular shifts in the skill mix of immigrants admitted to the United States). These recent studies, in effect, bring to the forefront the question of how cohort quality and immigrant self-selection are related. For example, are immigrants selected from the upper or lower tail of the ability (or income) distribution in the sending countries? Even if immigrants are drawn from the upper tail of the income distribution in the home country, does that ensure that they end up in the upper tail of the U.S. income distribution? Finally, if cohort quality has experienced a secular decline in the postwar period (as my

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[1] For a recent discussion of this identification problem, see James Heckman and Richard Robb (1983).

1985 analysis suggests), what factors are responsible for this change in the selection mechanism determining immigration?

This paper presents a theoretical and empirical study of these questions. It is assumed that individuals compare the potential incomes in the United States with the incomes in the home countries, and make the migration decision based on these income differentials (net of mobility costs). The use of this standard model allows a systematic analysis of the types of selection biases that are created by this behavior.[2] It will be seen that the common assumption that immigrants are drawn from the upper tail of the "home" income distribution requires a set of conditions that will not be generally satisfied. More importantly, this type of model suggests a few key variables (namely, the characteristics of the relevant income distributions) that "predict" the types of selection biases created by income-maximizing behavior on the part of potential migrants.

The empirical work presented in this paper analyzes the U.S. earnings of immigrants from forty-one countries using the 1970 and 1980 censuses. Not surprisingly, it is found that the variance in (relative) immigrant earnings across these countries is substantial. Using the theoretical insights, however, the analysis shows that the variance in various measures of the "quality" of immigrants can be explained to a large extent by a few key variables describing economic and political conditions in the countries of origin.

I. Theoretical Framework

Suppose there are two countries: country 0 and country 1. For concreteness, country 0 denotes the home country or the country of origin, while country 1 denotes the United States or the country of destination.[3] Residents of the home country have earnings which are distributed as

$$\ln w_0 = \mu_0 + \varepsilon_0, \tag{1}$$

where $\varepsilon_0 \sim N(0, \sigma_0^2)$. The earnings facing this population if they were to migrate to the United States are given by

$$\ln w_1 = \mu_1 + \varepsilon_1, \tag{2}$$

where $\varepsilon_1 \sim N(0, \sigma_1^2)$, and ε_0 and ε_1 have correlation coefficient ρ.

Equations (1) and (2) describe the earnings distributions facing a given individual that is contemplating emigration to the United States. This framework, due to A. D. Roy, can be interpreted as decomposing individual earnings into a part due to observable socioeconomic variables (μ_0 and μ_1), and a part due to unobserved characteristics (ε_0 and ε_1). The Roy model focuses on the impact of selection biases on the disturbances ε_0 and ε_1. Initially, therefore, variations in socioeconomic variables (which shift μ_0 and μ_1) are ignored, but their role will be discussed below.[4]

The parameter μ_1 is the mean income that residents from the home country would earn in the United States *if all* home country citizens were to migrate to the United States. In general, this level of income need not be the same as that of the U.S. native population since the average skills of the two populations—even in the absence of selection biases—may differ. For simplicity, in the remainder of the discussion it is assumed that these intercountry differences in skill (such as education and age) have been standardized, and hence μ_1 also gives the earnings of the average native worker in the United States.[5]

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[2] The model is formally identical to that presented in A. D. Roy's (1951) study of the impact of self-selection in occupational choice on the income distribution. The wealth-maximization hypothesis is also the cornerstone of the human capital model by Larry Sjaastad (1962). However, both Sjaastad's work and the literature it engendered pay little attention to the selection biases that are at the core of the Roy model.

[3] Two important problems are ignored by the two-country setup. First, it is likely that potential movers from any country j will have more than one possible country of destination. Second, the probability that U.S. native-born persons emigrate to other countries may not be negligible. These possibilities are ignored in order to focus on the essential aspects of the selection problem.

[4] The Roy model has been recently used by Robert Willis and Sherwin Rosen (1979) to analyze the types of selection biases created by the college attendance decision. Heckman and Guilherme Sedlacek (1985) present a generalization of the Roy model and apply it to the problem of estimating market wage functions.

The migration decision for persons in country 0 is determined by the sign of the index function:

$$(3)\qquad I=\ln(w_1/(w_0+C)) \approx (\mu_1-\mu_0-\pi)+(\varepsilon_1-\varepsilon_0),$$

where C gives the level of mobility costs, and π gives a "time-equivalent" measure of the costs of emigrating to the United States (i.e., $\pi=C/w_0$). Assume that π is constant across all individuals in the country of origin. Since migration to the United States occurs when $I>0$, the emigration rate from the country of origin is given by

$$(4)\qquad P=\Pr[v>-(\mu_1-\mu_0-\pi)] =1-\Phi(z),$$

where $v=\varepsilon_1-\varepsilon_0$; $z=-(\mu_1-\mu_0-\pi)/\sigma_v$; and Φ is the standard normal distribution function.

Equation (4) neatly summarizes the economic content of the theory of migration proposed by Larry Sjaastad. If follows from (4) that the emigration rate is: (a) a negative function of mean income in the home country; (b) a positive function of mean income in the United States; and (c) a negative function of the costs of emigrating to the United States. There are, however, a number of other implications in the theory that yield important insights into the kinds of selection biases generated by the endogenous migration decision. In particular, consider the conditional means $E(\ln w_0|I>0)$ and $E(\ln w_1|I>0)$. The first of these means gives the average earnings of emigrants in the country of origin, while the latter term gives the average earnings of these migrants in the United States. Under the normality assumptions these conditional means are given by

$$(5)\qquad E(\ln w_0|I>0)=\mu_0+\frac{\sigma_0\sigma_1}{\sigma_v}\left(\rho-\frac{\sigma_0}{\sigma_1}\right)\lambda,$$

$$(6)\qquad E(\ln w_1|I>0)=\mu_1+\frac{\sigma_0\sigma_1}{\sigma_v}\left(\frac{\sigma_1}{\sigma_0}-\rho\right)\lambda,$$

where $\lambda=\phi(z)/P$; and ϕ is the density of the standard normal. The variable λ is inversely related to the emigration rate, and takes on a value of zero when $P=1$ (James Heckman, 1979). Assume initially that $P<1$ so that at least part of the home country's population is better off by not emigrating. Then the second terms in (5) and (6) define the kinds of selection biases generated by income-maximizing behavior. Equation (5) shows that the average emigrant may be "better" or "worse" off than the average person in the country of origin depending on $\rho \gtrless \sigma_0/\sigma_1$. Similarly, equation (6) shows that the average immigrant in the United States may have higher or lower earnings than the average native person depending on $\sigma_1/\sigma_0 \gtrless \rho$. Let Q_0 be the income differential between the average emigrant and the average person in country 0, Q_1 be the income differential between the average immigrant and the average native person in the United States, and $k=\sigma_1/\sigma_0$. There are three cases that are of interest.[6]

Case 1: *Positive Selection*: $Q_0>0$ *and* $Q_1>0$. In this situation the "best" persons leave the country of origin and when they get to the United States, they outperform the native population. A reading of the literature on the earnings of immigrants suggests that this positive selection is most often assumed in the interpretation of those empirical results. Inspection of equations (5) and (6), however, shows that the necessary (and sufficient) conditions for positive selection to occur are

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[5] It is possible, of course, that the average person in country 0 has ethnic or racial characteristics which are favored or penalized by the U.S. labor market. Hence the mean income of (equally skilled) natives may not equal μ_1. This possibility is ignored in the discussion that follows, but it can be easily incorporated into the model.

[6] A fourth case where $Q_0>0$ and $Q_1<0$ is theoretically impossible since it requires $\rho>1$.

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$$(7) \qquad \rho > \min(1/k, k) \quad \text{and} \quad k > 1.$$

Thus if ρ is sufficiently high *and*, if income is more dispersed in the United States than in the country of origin, the immigrants arriving in the United States are indeed selected from the upper tail of the home country's income distribution and will outperform the native born.

Case 2: *Negative Selection*: $Q_0 < 0$ *and* $Q_1 < 0$. In this type of selection the United States draws persons from the lower tail of the home country's income distribution and these immigrants do not perform well in the U.S. labor market. The necessary (and sufficient) conditions for negative selection to occur are

$$(8) \qquad \rho > \min(1/k, k) \quad \text{and} \quad k < 1.$$

Negative selection again requires that ρ be "sufficiently" positive but that the income distribution be more unequal in the home country that in the United States.[7]

Case 3: *Refugee Sorting*: $Q_0 < 0$ *and* $Q_1 > 0$. The United States draws below-average immigrants (in terms of the country of origin), but they outperform the U.S. native born upon arrival. The necessary (and sufficient) condition for this to occur is

$$(9) \qquad \rho < \min(1/k, k).$$

These three cases summarize the quality differentials between migrants and the native base in each of the two countries. It seems plausible to argue that for non-Communist countries, ρ is likely to be positive and large. After all, profit-maximizing employers are likely to value the same factors in any market economy. The quality of immigrants in the United States then depends *entirely* on the ratio of variances in the income distributions of the United States and the country of origin. Suppose, for example, that $\sigma_0^2 > \sigma_1^2$. The United States, in a sense, "insures" low-income workers against poor labor market outcomes while "taxing" high-income workers (relative to the country of origin). This opportunity set implies that low-income workers have much greater incentives to migrate than high-income workers, and thus leads to immigrants being negatively selected from the population. Conversely, if $\sigma_1^2 > \sigma_0^2$, the home country now protects low-income workers from poor labor market outcomes and taxes the high-income worker. This opportunity set generates a "brain drain" into the United States. Available data on the distribution of income (World Bank, 1986, pp. 226–27) suggests that income is more unequally distributed in the large number of Third World countries (for example, Mexico, India, etc.) which form the bulk of current immigration to the United States.[8] Income-maximizing behavior is inconsistent with the traditional assumption that the United States draws the "best" workers from a given country and that those workers will (eventually) outperform the U.S. native born.

On the other hand, ρ need not always be positive and strong. It is likely, in fact, that ρ is negative for countries that have recently experienced a Communist takeover. The change from a market economy to a Communist system is often accompanied by structural shifts in the income distribution and, in particular, the confiscation of the financial holdings of entrepreneurs. Immigrants from such systems will be in the lower tail of the "revolutionary" income distribution but will outperform the average U.S. native worker. This result signals the movement of persons who cannot match with the new political structure, but who "seek refuge" and match quite well in a market economy.

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[7] The generalization of the model to allow for variable mobility costs (π) shows that the necessary conditions for negative (positive) selection remain unchanged as long as mobility costs and earnings do not have an "excessive" negative (positive) correlation. In addition, the impact of variable mobility costs on the results of the analysis is negligible if the variance in mobility costs is small relative to the variance in the income distributions.

[8] It must be noted, however, that these data on income inequality do not correspond exactly to the variances that are the primitive parameters of the Roy model. In particular, σ_0^2 and σ_1^2 describe the dispersion in "opportunities" (for a given socioeconomic characteristics).

These insights are developed under the assumption that selection biases *do* exist (i.e., $P<1$ and $\lambda>0$). Since for most countries in Latin America and Asia the mean level of the U.S. income distribution greatly exceeds the mean level of the home country's income distribution, it is unclear why—in the context of an income-maximizing model—the entire population of country 0 does not emigrate to the United States.

There are two reasons why we do not observe wholesale migrations of entire populations to the United States. First, it is not the differences in mean income levels that determine the extent of migration, but the differences in mean income levels *net* of migration costs. These migration costs will be both monetary and psychic, and are likely to be large in countries that have different cultural and social backgrounds than the United States. Second, there are statutory restrictions on the number of legal immigrants the United States will accept from any given country. These quotas play the important role of increasing migration costs of emigrants (if the numerical constraints are binding), since these individuals will presumably have to compete (and invest time and effort) to obtain the relatively scarce visas. Hence mobility costs ensure that only some persons in country 0 find it worthwhile to emigrate and thereby create the selection biases that are apparent in immigration data.

The model outlined above can be used to infer how the quality of immigrants in the United States will differ in the cross section (across different countries of origin) or over time (as economic conditions in the country of origin and in the United States change). The income-maximization hypothesis implies the existence of a reduced-form quality-of-immigrants equation given by

$$(10)\quad Q_1 = Q_1(\mu_1 - \mu_0 - \pi, \sigma_0, \sigma_1, \rho).$$

To determine the restrictions implied by the behavioral assumption of income maximization it is instructive to recall that $Q_1 = \gamma\lambda$, where $\gamma = (\sigma_0\sigma_1/\sigma_v)(k-\rho)$. The parameter γ does *not* depend on the size of the flow, while λ does. The impact of any variable α on the quality of immigrants in the United States is given by

$$(11)\qquad \frac{\partial Q_1}{\partial \alpha} = \lambda\frac{\partial \gamma}{\partial \alpha} + \gamma\frac{\partial \lambda}{\partial \alpha}.$$

The first term in (11) holds the size of the flow constant and will be called the "composition" effect. It measures how a change in the ability mix of a constant-sized immigrant pool affects their quality (relative to the U.S. native population). The second term in (11) will be called the "scale" effect and captures what happens to the quality of U.S. immigrants as the size of the flow is changed for any given "mix" (i.e., for constant γ).

Consider what happens to immigrant quality as the mean of the home country's income distribution increases. It can be shown that

$$(12)\qquad \frac{\partial Q_1}{\partial \mu_0} = \frac{\sigma_1\sigma_0}{\sigma_v^2}(k-\rho)\frac{\partial \lambda}{\partial z}.$$

Shifts in μ_0 lead only to a scale effect on Q_1. In addition, it is easy to show that $\partial\lambda/\partial z > 0$.[9] As discussed earlier, the sign of $k-\rho$ determines whether immigrants fall in the upper or lower tail of the U.S. income distribution. Equation (12) shows that $k-\rho$ also determines what happens to the (U.S.) earnings of immigrants as mean income in the home country increases. If $k-\rho$ is negative (immigrants are coming from countries with significantly more unequal income distributions and ρ is "sufficiently" positive), $\partial Q_1/\partial \mu_0 < 0$. The intuition for this result

[9] This follows trivially from the fact that λ is defined as $E(x|x>z)$, where x is a standard normal random variable.

follows from the fact that as μ_0 increases the emigration rate falls. The increase in μ_0 improves the position of the "marginal" immigrant so that he no longer migrates. But this marginal immigrant was more productive than the average immigrant. The increase in μ_0, therefore, leads to a reduction in the average quality of the immigrant population. Since the mean of the home country's income distribution and mobility costs play identical roles in the model, equation (12) also predicts that increases in mobility costs will *decrease* immigrant quality if $k-\rho<0$.

It is important to note that this result only captures the impact of changes in μ_0 (or migration costs) on the extent of selection bias (Q_1). The increase in μ_0, however, can be induced by either a shift in the skill distribution of the country of origin's population, or by an increase in the country's wealth that is unrelated to skills (for example, the discovery of a large inventory of natural resources). If μ_0 shifts because of the latter factor, equation (12) correctly predicts the change in observed immigrant earnings (which are given by μ_1+Q_1). However, if μ_0 shifts due to an increase in the skill level of the population, the change in immigrant earnings will also depend on the term $d\mu_1/d\mu_0$. This derivative will be positive if skills are transferable across countries, and this skill shift may dominate any changes that occur in Q_1 as μ_0 increases. Hence the (relative) earnings of immigrants in the United States may well be a positive function of μ_0 regardless of the sign of equation (12).[10]

The change in the quality of immigrants due to a mean-preserving increase in the income inequality of the home country is given by

$$(13)\quad \frac{\partial Q_1}{\partial \sigma_0}=\frac{\sigma_1^2\sigma_0}{\sigma_v^3}(\rho^2-1)\lambda - \frac{\sigma_1\sigma_0^2}{\sigma_v^3}(k-\rho)(1-\rho k)\frac{\partial\lambda}{\partial z}z,$$

where the first term gives the composition effect and the second term gives the scale effect. Since $|\rho|\leq 1$, the composition effect will always be nonpositive. An increase in σ_0 reduces the income of the poorest while it improves the position of the richest. Hence the mix of immigrants will include more persons from the lower tail of the distribution.

In addition, a change in σ_0 changes the rate of emigration. Equation (13) shows that the sign of the scale effect depends on the sign of three terms: $(k-\rho)$, $(1-\rho k)$, and z. The first two of these terms are nothing but the restrictions in equations (7) and (8). Suppose, for concreteness, that there is negative selection: the least-able persons leave the home country and they perform below the U.S. native average. This implies $k-\rho<0$ and $1-\rho k>0$. Inspection of (13) reveals that the direction of the scale effect depends on the sign of $z=-(\mu_1-\mu_0-\pi)/\sigma_v$. If $\mu_1>\mu_0+\pi$, so that mean U.S. incomes are higher than foreign incomes even after adjusting for mobility costs, z is negative, the scale effect is negative, and thus immigrants from countries with more income inequality will perform worse in the United States.

The intuition for the workings of the scale effect can be grasped by considering Figure 1, which is drawn with $z<0$ and $\sigma_1<\sigma_0$. As σ_0 increases, the worst-off persons in country 0 will still want to migrate, while the better-off persons become relatively better

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[10] This discussion illustrates how differences in skill characteristics can enter the Roy model. More generally, the earnings distributions in the two countries can be written as

$$\ln w_0 = X\delta_0+\varepsilon_0,\quad \ln w_1 = X\delta_1+\varepsilon_1,$$

and the emigration rate (for given characteristics X) is given by

$$P=\Pr\{(\varepsilon_1-\varepsilon_0)>-[X(\delta_1-\delta_0)-\pi]\}.$$

Selection will occur not only on the basis of unobserved characteristics (ε), but also in terms of the socioeconomic variables X as long as the two countries value these skills differently. Although the empirical analysis below (by holding X constant) focuses on the selections in ε, it would be very interesting to also investigate the types of selections generated in X.

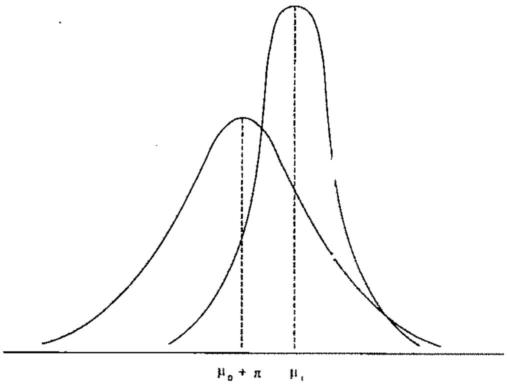

FIGURE 1

off and their migration incentives decline. The emigration rate drops due to the withdrawal of the "best" potential migrants from the market, and thus the quality of the pool that does reach the United States declines.

The last characteristic of the home country's income distribution which determines the quality of immigrants is ρ. It can be shown that

$$(14)\quad \frac{\partial Q_1}{\partial \rho} = -\frac{\sigma_1\sigma_0^3}{\sigma_v^3}(1-\rho k)\lambda + \frac{\sigma_1^2\sigma_0^2}{\sigma_v^3}(k-\rho)\frac{\partial\lambda}{\partial z}z.$$

Changes in the correlation coefficient also induce two effects. Consider first the composition effect. Its sign depends on $-(1-\rho k)$, which is negative if there is negative selection. An increase in ρ implies that a better match exists between performance in the United States and in the home country. Since $\sigma_0 > \sigma_1$ this decreases the profitability of migration for the best persons in country 0 and increases it for the worst persons.

In addition, changes in ρ have an impact on the emigration rate, and the scale effect is given by the last term in (14). If the conditions for negative selection hold, $k-\rho<0$ and the sign of the scale effect will depend on the sign of $-z$. If, as before, we assume $z<0$, the scale effect of an increase in the correlation coefficient on the quality of immigrants is seen to be positive.

A summary of the comparative statics results under the various regimes is provided in Table 1. One implication is immediately clear: generalizations about the quality of immigrants in the United States are hard to come by. The model does, however, isolate the key factors that determine the types of selections in the immigrant population and these factors shed some light on my 1985 finding that the quality of immigrants declined in the postwar period. Prior to the 1965 Amendments to the Immigration and Nationality Act, immigration to the United States from Eastern Hemisphere countries was regulated by numerical quotas. These quotas were based on the ethnic population of the United States in 1919 and thus encouraged immigration from (some) Western European countries and discouraged immigration from all other countries. The favored countries have one important characteristic: their income distributions are probably much less dispersed than those of countries in Latin America or Asia. The 1965 Amendments revamped the quota system, established a 20,000 numerical limit for immigration from any single country (subject to both hemispheric and worldwide numerical limits), and led to a substantial increase in the number of immigrants from Asia and Latin America. The new flow of migrants originate in countries that are much more likely to have greater income inequality than the United States. It would not be surprising, therefore, if the quality of immigrants declined as a result of the 1965 Amendments.[11]

II. Empirical Framework

The quality measure Q_1 derived in the previous section is the standardized wage differential between immigrants and natives

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[11] In addition, the 1965 Act changed the emphasis in the allocation of visas toward family reunification and away from occupational preferences. This shift may well lead to an even steeper decline in the quality of immigrants admitted to the United States.

TABLE 1—SUMMARY OF COMPARATIVE STATICS RESULTS

		Positive Selection $Q_0 > 0, Q_1 > 0$	Negative Selection $Q_0 < 0, Q_1 < 0$	Refugee Sorting $Q_0 < 0, Q_1 > 0$
$\partial Q_1/\partial \mu_0$:	Composition Effect	none	none	none
	Scale Effect	+	−	+
$\partial Q_1/\partial \sigma_0$:	Composition Effect	−	−	−
	Scale Effect, $z < 0$	−	−	+
	$z > 0$	+	+	−
$\partial Q_1/\partial \rho$:	Composition Effect	+	−	−
	Scale Effect, $z < 0$	−	+	−
	$z > 0$	+	−	+

in the United States. In any given cross section, this wage differential is affected by two factors: 1) differences in the skill composition of the various immigrant cohorts; and 2) the rate of convergence between foreign- and native-born earnings (i.e., the rate of assimilation of immigrants). An empirical framework for measuring these effects thus begins with the specification of the regression model:

$$(15)\quad \ln w_i(T) = X_i\theta_T + \delta I_i + \alpha_1 I_i y_i + \alpha_2 I_i y_i^2 + \beta_1 I_i C_i + \beta_2 I_i C_i^2 + v_i,$$

where $w_i(T)$ is the wage rate of individual i in cross-section year T; X_i is a vector of socioeconomic characteristics; I_i is a dummy variable set to unity if the individual is foreign born; y_i represents the number of years the immigrant has resided in the United States; and C_i is the calendar year of the immigrant's arrival. The parameters α_1 and α_2 capture the impact of assimilation on the (relative) earnings of immigrants, while β_1 and β_2 capture the cohort differentials.[12]

Of course, in a single cross section of data, equation (15) cannot be estimated since the variables C_i and y_i are related by the identity $T \equiv C_i + y_i$. Substituting this identity in (15) yields

$$(16)\quad \ln w_i(T) = X_i\theta_T + (\delta + \beta_1 T + \beta_2 T^2) I_i + (\alpha_1 - \beta_2 - 2\beta_2 T) I_i y_i + (\alpha_2 + \beta_2) I_i y_i^2 + v_i.$$

Equation (16) shows that the typical cross-section earnings function estimated in the immigration literature does not identify a single parameter of interest.[13] It is easy to show, however, that if another cross section is available in calendar year T' all the parameters in (15) can be identified.[14] Moreover, the comparison of the two cross-section regressions provides interesting insights about the extent and direction of cohort-quality differentials. Let $\gamma_1 = \delta + \beta_1 T + \beta_2 T^2$, $\gamma_2 = \alpha_1 - \beta_1 - 2\beta_2 T$, and $\gamma_3 = \alpha_2 + \beta_2$, be the coefficients of the immigration variables in the cross section at calendar year T.

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[12] The parameters β_1 and β_2 capture two kinds of cohort effects: 1) differences in the skill composition of cohorts due to a secular trend in the quality of immigrants; and 2) differences due to selective emigration of foreign-born persons in the United States. Little is known, however, about the selection biases associated with return migration even though the Roy model can be generalized to account for the possibility that individuals make "mistakes." Unfortunately, U.S. data on the return migration of foreign-born persons is basically nonexistent, and hence this problem is ignored in what follows.

[13] Except perhaps for the coefficient of the immigrant dummy. This coefficient gives the wage differential between the most recent cohort of immigrants and the native-born population.

[14] Provided that period effects on the immigrant-native wage differential are negligible. This assumption is far from innocuous. Unfortunately, since only two cross sections are available, little can be done to test its validity

This vector will shift over time since

$$\partial \gamma_1/\partial T = \beta_1 + 2\beta_2 T \tag{17}$$

$$\partial \gamma_2/\partial T = -2\beta_2 \tag{18}$$

$$\partial \gamma_3/\partial T = 0 \tag{19}$$

The immigration vector in cross-section earnings functions (except for the coefficient of $I \cdot y^2$) is inherently unstable, though the direction of the instability provides insights into the underlying structural changes. For instance, γ_1 (the coefficient of the immigrant dummy) will be shifting down over time if the quality of immigrants is decreasing at the "margin" (i.e., in the cross-section year T). In addition, the age-earnings profile of immigrants (relative to natives) becomes steeper over time (i.e., γ_2 increases) if the decline in the quality of immigrant cohorts has accelerated over the sample period.

The empirical analyses below uses the 1970 and 1980 census cross sections to identify the parameters of interest (δ, α_1, α_2, β_1, and β_2). From these estimates it is possible to calculate measures of three alternative dimensions of cohort quality that underlie the discussion. The first of these dimensions is simply the wage of an immigrant cohort relative to the native base prior to any assimilation taking place; that is, a measure of the "raw" skills a given immigrant cohort brings to the United States. A second dimension is given by the extent to which the quality of successive immigrant cohorts is changing over time, while a third dimension is given by the extent to which the earnings of a specific immigrant cohort grow—above and beyond pure aging effects—in the U.S. labor market. Clearly, there are many ways of defining variables that capture these three facets of the "quality" of immigrants. However, since all possible definitions of a particular dimension of quality are based on the same underlying parameters, there is a high degree of correlation among the alternative measures. Thus, to some extent, the choice of the empirical representation of a given facet of quality is arbitrary. In the empirical analysis below, the three dimensions of quality are defined by

1) The predicted wage differential in 1979 between the most recently arrived immigrant cohort and the native base. This measure of the quality of a single cohort of immigrants—prior to assimilation taking place—is given by the coefficient of the immigrant dummy variable in the 1980 cross section.

2) The rate of wage growth (relative to natives) for an immigrant cohort that has resided in the United States for ten years. This is the assimilation effect evaluated at $y = 10$, and is given by $\partial \ln w/\partial y|_{y=10} = \alpha_1 + 20\alpha_2$.

3) The predicted wage differential immediately after immigration between the 1979 cohort and the 1955 cohort. This measure of the extent of cohort-quality change is designed to compare the typical immigrant that migrated prior to the 1965 Immigration and Nationality Act with the typical immigrant from the most recent wave. Using equation (15) it is easy to show that this change in cohort quality is given by $24(\beta_1 + 2\beta_2 T - 24\beta_2)$, where T indexes the 1980 cross section.

III. Regression Results from the 1970–1980 Censuses

The data are drawn from the 1970 2/100 U.S. Census (obtained by pooling the 5% SMSA and County Group Sample and the 5% State Sample) and the 1980 5/100 A Sample.[15] The complete samples are used in the creation of the immigrant extracts, but random samples are drawn for the native "baseline" population.[16] The analysis is restricted to men aged 25–64 who satisfied four sample-selection rules: 1) the individual was employed in the calendar year prior to the census; 2) the individual was not self-employed or working without pay; 3) the individual was not in the Armed Forces (as of the survey week); and 4) the individual did not reside in group quarters.[17]

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[15] The two 1970 samples that are pooled are essentially independent of each other. The only substantive difference between them—in the context of this study—is that the set of persons for whom SMSA residence is defined differs in the two samples. However, the coefficients of the SMSA dummy in earnings functions estimated separately in the two samples are not statistically different from each other.

[16] The native-born extract is a .001 sample in the 1970 census and a .00042 sample in the 1980 census.

Since labor market conditions changed substantially between 1970 and 1980, the empirical framework derived in the previous section focused on the behavior of immigrant earnings *relative* to the earnings of natives. In this paper *all* immigrant groups will be compared to a single native base: the group of white, non-Hispanic, non-Asian men.

Forty-one countries were chosen for analysis. The countries were selected on the basis that *both* the 1970 and 1980 censuses contained a substantial number of immigrants from the country. In particular, it is necessary to have at least 80 observations of persons born in a particular foreign country in the pooled 2/100 1970 Census to enter the sample of the 41 countries.[18] The 41 countries under analysis account for 90.4 percent of all immigration to the United States between 1951 and 1980.

Summary statistics on the immigrant flow in the 1951–80 period are presented in Table 2. The first column of Table 2 gives the total number of immigrants from each country that arrived in the United States in that period. Although this number is interesting, it is more instructive if it is converted into a percentage of the country of origin's 1980 population. This statistic is presented in the second column and gives the percentage by which the country of origin's population would increase (in 1980) if all the persons who emigrated to the United States in the past three decades returned to their birthplace. This percent ranges from the trivially small (.04 percent of Brazil and the USSR) to the amazingly large (over 10 percent for Jamaica). Of the 41 countries in Table 2, 17 of them experienced emigration to the United States which exceeded 1 percent of that country's population.

The national composition of the flows received by the United States over the 1951–80 period did not remain constant over the three decades. The third column gives the flow of immigrants in the 1951–60 decade as a percent of the country's 1950 population; while the fourth column presents the flow of immigrants in 1971–80 as a percent of the country's 1970 population. These statistics document the declining importance of Western European countries as a source of immigrants and the increasing importance of Asia and Latin America. The fact that the characteristics of the sending countries changed drastically during the postwar period implies that the types of selections that distinguish the immigrant population from the native born also changed.

The 1970 and 1980 cross-section regressions were jointly estimated in each of the 41 samples (i.e., the group of immigrants from a specific country of origin pooled with the "white" native base), using the (ln) wage rate in the year preceding the census as the dependent variable. The socioeconomic vector of characteristics X included: years of completed schooling, age, age squared, whether health limits work, whether married, spouse present, and whether resident of an SMSA. The regression framework derived in Section II implies that the coefficient of the quadratic years-since-migration variable should be constant across censuses. This restriction was satisfied (at the 5 percent level of significance) by 32 of the 41 countries in the data, and hence was imposed on the analysis.

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[17] The analysis is restricted to men aged 25–64 in either census year. This differs from the more common methodology of tracking the "same" men over time. It can be shown (Heckman and Robb) that if the underlying parameters are constant over time, it is unnecessary to track specific cohorts across censuses in order to identify the structure. In addition, the samples exclude men who are self-employed. This restriction creates its own set of selection biases. However, an equally serious problem would arise if self-employed men were included in the study and their incomes were analyzed jointly with the wages of salaried men. Finally, the data exclude men who had annual earnings under $1,000 in either of the census years.

[18] Only two of the countries in the analysis have between 80 to 100 observations in the 1970 census, an additional 11 countries have between 101 and 200 observations, 8 have between 201 and 300 observations, and 20 have more than 300 observations. Of course, the sample sizes in the 1980 census are significantly larger.

TABLE 2—IMMIGRATION FLOWS TO THE UNITED STATES IN THE 1951–80 PERIOD

	1951–80 Immigration		1951–60 Immigrants as Percent of 1950 Population[a]	1971–80 Immigrants as Percent of 1970 Population[a]
Country of Birth	Total Number (in 1000s)	As Percent of 1980 Population[a]		
Europe:				
Austria	48.1	.6	.4	.1
Czechoslovakia	60.4	.4	.2	.1
Denmark	30.0	.6	.3	.1
France	90.1	.2	.1	.04
Germany	611.5	1.0	.7	.1
Greece	232.3	2.4	.6	1.1
Hungary	93.4	.9	.7	.1
Ireland	120.9	3.5	2.2	.5
Italy	524.8	.9	.4	.2
Netherlands	85.7	.6	.5	.1
Norway	45.1	1.1	.8	.1
Poland	244.9	.7	.5	.1
Portugal	204.2	2.1	.2	1.2
Romania	49.8	.2	.1	.1
Spain	71.2	.2	.04	.1
Sweden	41.9	.5	.3	.1
Switzerland	40.1	.6	.4	.1
United Kingdom	562.9	1.0	.4	.2
USSR	105.4	.04	.02	.02
Yugoslavia	147.0	.7	.4	.2
Asia and Africa:				
China (Taiwan)	331.9	1.9	.4	1.4
Egypt	46.4	.1	.02	.1
India	211.1	.03	.001	.03
Iran	59.1	.2	.01	.2
Israel	48.1	1.3	.7	.9
Japan	131.1	.1	.05	.05
Korea	314.8	.8	.02	.8
Philippines	478.9	.9	.1	1.0
Americas:				
Argentina	81.5	.3	.1	.3
Brazil	43.1	.04	.02	.01
Canada	676.4	2.8	2.0	.5
Colombia	165.5	.6	.4	.6
Cuba	611.9	6.3	1.5	3.2
Dominican Republic	251.9	4.3	.5	3.4
Ecuador	96.7	1.2	.3	.8
Guatemala	45.1	.7	.1	.5
Haiti	100.2	1.8	.1	1.3
Jamaica	221.7	10.3	.6	7.3
Mexico	1399.8	2.0	1.2	1.3
Panama	50.8	2.6	1.2	1.5
Trinidad & Tobago	88.0	8.0	.2	6.0

Source: U.S. Bureau of the Census (various issues).

[a] The population base refers to the country of origin.

The restricted coefficients of the immigration vector in both the 1970 and 1980 census cross sections are presented in the first five columns of Table 3. The coefficients of the immigration variables in 1970 differ drastically from the coefficients of the immigration variables in 1980. This difference implies that cross-section regressions do not capture the "true" assimilation impact since cohort effects are confounding the analysis. Consider, for example, Colombia: In 1970, the most recent immigrants earned about 22 per-

TABLE 3—ESTIMATES OF MODEL PARAMETERS[a]

Country of Birth	1970		1980			Rate of Assimilation	1955–79 Change in
	I	$I\cdot y$	I	$I\cdot y$	$I\cdot y^2$	at $y=10$	Cohort Quality
Europe:							
Austria	.0189	.0036	.0321	.0034	−.00003	.0040	.0287
	(.26)	(.75)	(.52)	(.82)	(−.45)	(.66)	(.20)
Czechoslovakia	−.1525	.0147	−.1441	.0127	−.00019	.0088	−.0143
	(−2.48)	(3.34)	(−2.79)	(3.23)	(−2.74)	(1.64)	(−.10)
Denmark	.0838	−.0033	.2018	−.0056	.00009	.0068	.2441
	(.82)	(−.44)	(2.14)	(−.81)	(.72)	(.78)	(1.21)
France	−.0785	.0020	.0999	−.0046	.00005	.0111	.3183
	(−1.28)	(.47)	(2.48)	(−1.33)	(.79)	(2.05)	(2.74)
Germany	.0999	−.0025	.1409	−.0047	.00007	−.0002	.0618
	(3.82)	(−1.37)	(5.40)	(−2.62)	(2.38)	(−.10)	(1.17)
Greece	−.2400	.0115	−.3092	.0141	−.00018	.0049	−.1231
	(−6.70)	(3.73)	(−11.28)	(5.42)	(−3.33)	(1.56)	(−1.75)
Hungary	−.1555	.0173	−.2082	.0145	−.00021	.0036	−.1744
	(−2.98)	(4.12)	(−4.30)	(4.23)	(−3.31)	(.86)	(−1.85)
Ireland	−.0732	.0019	−.0514	.0027	−.00002	.0050	.0666
	(−1.54)	(.53)	(−1.09)	(.78)	(−.28)	(1.26)	(.72)
Italy	.0133	.0060	−.0673	.0065	−.00009	−.0031	−.1855
	(.60)	(3.72)	(−3.45)	(4.58)	(−3.49)	(−1.55)	(−4.07)
Netherlands	.0127	−.0061	.1252	−.0074	.00015	.0062	.2487
	(.23)	(−1.45)	(2.71)	(−2.15)	(2.35)	(1.35)	(2.41)
Norway	.2245	−.0093	.2785	−.0096	.00015	−.0013	.1241
	(2.54)	(−1.55)	(3.77)	(−1.76)	(1.58)	(−.17)	(.71)
Poland	−.1936	.0181	−.2734	.0184	−.00024	.0058	−.1865
	(−5.70)	(7.62)	(−11.08)	(9.61)	(−6.86)	(1.98)	(−3.08)
Portugal	.0797	.0032	−.0913	.0073	−.00012	−.0102	−.3418
	(1.95)	(.86)	(−3.25)	(2.47)	(−1.95)	(−2.77)	(−4.02)
Romania	−.3015	.0263	−.3161	.0229	−.00030	.0136	−.0929
	(−4.23)	(4.97)	(−7.02)	(5.47)	(−3.65)	(2.17)	(−.72)
Spain	−.3547	.0233	−.1920	.0134	−.00022	.0203	.2245
	(−6.15)	(4.32)	(−4.10)	(2.88)	(−2.39)	(3.98)	(1.92)
Sweden	.0128	.0119	.0465	.0099	−.00021	.0080	.0465
	(.13)	(1.90)	(.69)	(1.88)	(−2.14)	(.88)	(.24)
Switzerland	−.0201	.0132	.1467	.0067	−.00015	.0171	.2912
	(−.27)	(2.18)	(2.48)	(1.33)	(−1.56)	(2.56)	(1.97)
United Kingdom	.0607	−.0006	.1271	−.0023	.00002	.0038	.1303
	(2.70)	(−.34)	(7.38)	(−1.61)	(.67)	(1.84)	(2.81)
USSR	−.3509	.0277	−42.99	.0262	−.00035	.0105	−.2144
	(−6.70)	(8.34)	(−18.75)	(11.70)	(−7.67)	(2.22)	(−2.31)
Yugoslavia	−.0659	.0096	−.0920	.0097	−.00009	.0054	−.0608
	(−1.51)	(2.72)	(−2.82)	(3.52)	(−1.61)	(1.49)	(−.79)

cent less than the native base, and their (relative) earnings increased by about 1.7 percent in the first year after immigration. By 1980, the most recent wave of Colombians earned 40 percent less than the *same* native base, and their earnings increased by about 2.2 percent in the first year after immigration. The tilting of the cross-section profile so that later cross sections are steeper and have a more negative constant term implies that the quality of the more recent Colombian immigrant waves is lower than that of the earlier waves. Conversely, consider the immigrants from France: In 1970, the typical French immigrant earned about 8 percent less than a comparable native person, and had earnings growth of about .2 percent during that first year after immigration. By 1980, the most recent immigrant earned about 10 percent *more* than the native base, and had earnings growth of minus .5 percent during that first year. The flatten-

TABLE 3—CONTINUED

	1970		1980			Rate of Assimilation at $y=10$	1955–79 Change in Cohort Quality
Country of Birth	I	$I\cdot y$	I	$I\cdot y$	$I\cdot y^2$		
Asia and Africa:							
China (Taiwan)	−.4525	.0227	−.5327	.0254	−.00037	.0114	−.1481
	(−14.34)	(9.43)	(−26.43)	(11.66)	(−8.22)	(4.01)	(−2.44)
Egypt	−.4466	.0421	−.4586	.0396	−.00056	.0260	−.0706
	(−7.00)	(5.67)	(−10.84)	(7.57)	(−4.34)	(4.76)	(−.57)
India	−.2847	.0453	−.4340	.0497	−.00096	.0179	−.2845
	(−7.09)	(9.71)	(−21.41)	(16.75)	(−11.03)	(5.33)	(−3.84)
Iran	−.4078	.0229	−.3101	.0249	−.00031	.0294	.2690
	(−4.71)	(3.03)	(−10.19)	(5.45)	(−2.47)	(4.13)	(1.88)
Israel	−.2998	.0282	−.3397	.0260	−.00041	.0128	−.1314
	(−4.19)	(4.54)	(−8.44)	(5.74)	(−3.84)	(2.11)	(−1.00)
Japan	−.1314	.0010	.1016	−.0049	.00002	.0159	.4616
	(−2.65)	(.19)	(4.31)	(−1.46)	(.18)	(3.60)	(4.78)
Korea	−.5450	.0439	−.4481	.0393	−.00071	.0323	.1544
	(−8.69)	(5.72)	(−19.44)	(9.68)	(−5.40)	(6.31)	(1.37)
Philippines	−.4360	.0265	−.3881	.0266	−.00041	.0233	.1158
	(−13.31)	(11.30)	(−23.14)	(13.33)	(−9.34)	(7.84)	(1.80)
Americas:							
Argentina	−.2099	.0210	−.2427	.0186	−.00032	.0077	−.1191
	(−3.81)	(3.58)	(−5.80)	(4.13)	(−3.11)	(1.65)	(−1.12)
Brazil	−.1430	.0114	−.0257	.0062	−.00015	.0123	.1941
	(−1.70)	(1.44)	(−.45)	(1.00)	(−1.11)	(1.66)	(1.19)
Canada	.0645	.0003	.1165	−.0013	−.00000	.0030	.0988
	(2.86)	(.17)	(6.06)	(−.91)	(−.21)	(1.50)	(2.17)
Colombia	−.2247	.0169	−.4030	.0219	−.00036	−.0007	−.3444
	(−4.33)	(2.74)	(−12.67)	(5.78)	(−3.71)	(−.17)	(−3.82)
Cuba	−.4612	.0214	−.4517	.0208	−.00025	.0164	.0129
	(−22.20)	(8.89)	(−18.26)	(9.24)	(−5.20)	(9.74)	(.28)
Dominican Republic	−.3293	.0141	−.4556	.0142	−.00018	−.0019	−.3020
	(−5.81)	(2.45)	(−13.91)	(3.62)	(−1.74)	(−.44)	(−3.01)
Ecuador	−.4041	.0242	−.4195	.0210	−.00026	.0127	−.0906
	(−6.06)	(3.28)	(−9.77)	(4.13)	(−1.98)	(2.58)	(−.82)
Guatemala	−.5127	.0408	−.4013	.0298	−.00066	.0222	.0828
	(−5.76)	(5.03)	(−8.97)	(5.09)	(−4.40)	(2.96)	(.51)
Haiti	−.3356	−.0027	−.5234	.0175	−.00011	.0064	−.1130
	(−4.99)	(−.34)	(−13.95)	(3.39)	(−.77)	(1.20)	(−.94)
Jamaica	−.3322	.0165	−.2594	.0097	−.00020	.0095	.0600
	(−6.75)	(4.06)	(−9.33)	(2.92)	(−2.77)	(2.24)	(.64)
Mexico	−.3307	.0191	−.4037	.0206	−.00031	.0078	−.1497
	(−16.57)	(14.80)	(−34.72)	(22.25)	(−15.94)	(4.16)	(−3.61)
Panama	−.3438	.0159	−.2516	.0115	−.00010	.0165	.1476
	(−3.52)	(2.31)	(−4.35)	(2.07)	(−.88)	(2.04)	(.84)
Trinidad & Tobago	−.3091	.0187	−.3257	.0211	−.00024	.0158	.0013
	(−4.02)	(2.59)	(−6.94)	(3.70)	(−1.95)	(2.35)	(.03)

[a] The t-ratios are presented in parentheses. The cross-section regressions hold constant the individual's completed schooling, age, marital status, health, and SMSA residence.

ing of the cross-section profile implies that the quality of French immigrants increased over the sample period.

Three dimensions of cohort quality are implicit in these regression coefficients. The entry wage differential between the 1979 immigrant cohort and the native base is given by the coefficient of the immigrant dummy in the 1980 census cross section. Table 3 clearly shows that this coefficient has a large variance across countries. The last two columns of Table 3 present estimates of the other two dimensions of cohort quality: the assimilation rate defined by the slope of the earnings-assimilation path at $y=10$; and the rate of change in cohort quality, defined by the earnings differential between the 1979 cohort and the 1955 cohort at the time of arrival in the United States. Since these estimated parameters are functions of the cross-section coefficients of Table 3, it is not surprising to find that there is a lot of variance in both of these variables across countries. Immigrants from some countries have high assimilation rates, while immigrants from other countries experience no assimilation at all. Similarly, the rate of cohort-quality change is sometimes positive (thus indicating quality increased between the 1955 and 1979 cohorts) and sometimes negative (thus indicating a quality decrease across cohorts). For example, the most recent immigrant wave from the United Kingdom has an earnings potential that is about 13 percent higher that the wave that arrived in 1955, while the most recent immigrants from India have 28 percent lower earnings than the earlier cohort.

The Roy model suggests that country-specific characteristics of the income distribution (and mobility costs) determine the quality of immigrants in the United States. The important task, therefore, becomes the identification of observable variables which can proxy for these theoretical parameters, and the determination of whether these country-specific variables "explain" the variance in the quality proxies presented in Table 3.

IV. Determinants of Immigrant Quality

Table 4 describes the construction and source of country-specific aggregate variables which portray the political and economic conditions (as well as some characteristics of the immigrant populations) of the 41 countries under analysis during the 1950–79 period. Table 4 also presents the mean and range of these variables and comparable statistics for the United States.

Three of these variables are designed to capture political conditions in the country of origin. These political measures are obtained from the Cross-National Time-Series Archive (CNTSA), a historical data set containing both political and economic variables for all sovereign countries since 1815 (up to 1973).[19] The CNTSA set contains a variable describing the extent of "party legitimacy," that is, whether or not there is competition among political parties in the electoral system. The measure of party legitimacy is interpreted as an index of political freedom, and is used to construct two variables: 1) a dummy variable set equal to unity if the immigrant's birthplace had a competitive political system during the *entire* 1950–73 period; and 2) a dummy variable set equal to unity if the immigrant's birthplace had a competitive political system at the beginning of the period, but lost its political freedom by the end of the period. The omitted dummy variable indicates whether the birthplace of the immigrant had a noncompetitive political system both at the beginning and at the end of the 1950–73 period.[20] The last index of political stability used is a variable measuring the number of political assassinations (defined as a politically motivated murder or attempted murder of a high government official or politician) that took place in the specific country during the 1950–73 period.

The country-specific vector also includes variables that describe economic conditions in the various countries of origin:

1) The logarithm of per capita Gross National Product in 1980 (in U.S. dollars). In addition, the analysis also uses the average annual percentage change in that variable over the 1963–80 period. These variables, of course, are designed to control for the mean level of the income distribution (as well as changes in that level) in the various countries of origin.

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[19] The CNTSA was created by Arthur Banks and is available through the Inter-University Consortium for Political and Social Research, Ann Arbor, MI 48106.

[20] There is also the possibility that a country gained its freedom during the 1950–73 period. Only one country, however, falls in this category (the Dominican Republic). To reduce the number of exogenous variables, this country was pooled with the countries that were "free" throughout the entire period.

TABLE 4—DEFINITION OF COUNTRY-SPECIFIC VARIABLES

Variable	Definition and Source	Mean	Minimum	Maximum	U.S. Value
Politically Competitive System	=1 if the country had a competitive party system during the entire 1950–73 period; 0 otherwise. *Source*: Cross-National Time-Series Archive (CNTSA)	.41	–	–	1
Recent Loss of Freedom	=1 if the country had a competitive party system at the beginning of the period but had a noncompetitive party system at the end of the period; 0 otherwise. *Source*: CNTSA.	.20	–	–	0
Number of Assassinations	Number of politically motivated murders or attempted murders of high government officials or politicians in 1950–73. *Source*: CNTSA.	3.27	0	22	12
Income Inequality	Ratio of household income of the top 10 percent of the households to the income of the bottom 20 percent of the households. *Source*: World Bank (various issues) and United Nations (1977).	7.50	1.42	30.0	5.91
Distance from U.S.	Number of air miles (in thousands) between the country's capital and the nearest U.S. gateway (Los Angeles, Miami, or New York). *Source*: Airline offices contacted by author.	3.37	.18	7.49	–
English Proficiency	Fraction of 1975–80 cohort of immigrants who speak English well or very well. *Source*: 5/100 A Sample of the 1980 U.S. Census.	.74	.24	1.00	–
Age at Migration	Mean age at migration. *Source*: 5/100 A Sample of the 1980 U.S. Census.	24.56	12.40	32.40	–
ln (per capital GNP)	(ln) 1980 per capita GNP in dollars. *Source*: U.S. Arms Control and Disarmament Agency (1984).	8.17	5.42	9.62	9.39
Rate of Change in Per Capita GNP	Annual rate of change in per capita GNP between 1963 and 1980, defined by: $\ln(GNP_{1980}/GNP_{1963})/17$. *Source*: U.S. Arms Control and Disarmament Agency (1975, 1984).	.03	.004	.07	.02
Rate of Change in Central Government Expenditures	Annual Change in the Percentage of GNP that is accounted for by central government expenditures, defined by $(GOVT_{1980} - GOVT_{1950})/30$, where $GOVT_t$ is the percent of GNP attributable to central government expenditures in year t. *Source*: CNTSA and U.S. Arms Control and Disarmament Agency (1984).	.41	−1.69	2.08	.26
Change in Quota	Change in fraction of population eligible for migration to the U.S., defined by $(20000/1979 \text{ population}) \div (QUOTA/1950 \text{ population})$, where 20,000 is the maximum number of visas allocated to the country after 1965, and $QUOTA$ is the number of visas allocated prior to 1965. *Source*: U.S. Immigration and Naturalization Service (1965).	38.90	.28	149.67	–

2) The ratio of household income accruing to the top 10 percent of the households to the income accruing to the bottom 20 percent of the households (circa 1970). Unfortunately, this measure of income inequality does not exist prior to the 1970's for most of the countries under analysis, and hence the change in the extent of income inequality during the last three decades cannot be documented. The empirical analysis below will proxy for the change in income inequality by using the change in the fraction of GNP that can be attributed to central government expenditures over that period.

Presumably, the greater the role of the government the more taxation and income redistribution that occurs, and hence the less unequal the income distribution will be.

3) The level of mobility costs is proxied by the number of air miles between the country's capital and the nearest U.S. gateway.

Finally, the regressions also include variables that describe relevant characteristics of the immigrant population itself. The two variables in this category are the fraction of the most recently arrived immigrants who speak English well or very well, and the average age at migration. These variables are likely to affect the earnings of immigrants as well as their incentives to invest in human capital, and hence will be important determinants of immigrant quality.

A. *Determinants of the Entry Wage Differential*

In the last section a variable measuring the entry wage differential between the foreign born and the native born for the immigrant cohort arriving in 1979 was calculated for each of the 41 countries under analysis. Table 5 presents the generalized least squares regressions of this measure of immigrant quality on the country-specific aggregate variables.[21]

The simplest specification in column 1 shows that the variable measuring whether or not the country was politically competitive in the postwar period has a strong positive impact on the immigrant's entry wage. Immigrants from these countries have 27 percent higher relative earnings (at the time of entry into the United States) than immigrants from politically repressive countries. This basic regression also shows that the extent of income inequality has a weak negative impact on the relative quality of immigrants. Immigrants from countries with more income inequality are of lower quality. This result is consistent with the theoretical implications of the Roy model. As income inequality increases, the migration incentives for persons in the upper tail of the distribution decline, thus lowering the average quality of the immigrant population.

In column 2 of Table 5, the variables measuring mobility costs and the age and English proficiency of immigrants are added to the regression. The results suggest that persons migrating from countries that have 100 percent English proficiency rates have about 26 percent higher relative earnings at the time of entry than immigrants from countries with 0 percent English proficiency rates.[22] Table 5 also shows that age at migration has a significant *negative* impact on the initial relative earnings of immigrants in the United States. Hence persons who migrate as youths have an easier time in the U.S. labor market than older immigrants.

The third regression in Table 5 adds the level of *GNP* per capita to the list of exogenous variables. Its impact is strongly positive, and its inclusion increases the explanatory power of the regression to over 80 percent. A 10 percent increase in a country's GNP increases the relative earnings of

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[21] The dependent variables in the "second-stage" regressions presented in this section are themselves estimated regression coefficients (or linear combinations thereof). Hence the disturbances in these regressions are heteroscedastic. Let y_i be the true value of the dependent variable (for country i) in the second-stage regressions. The "true" model is given by $y_i = Z_i\beta + \varepsilon_i$, where $E(\varepsilon_i) = 0$ and $E(\varepsilon_i^2) = \sigma_\varepsilon^2$. The variable y_i is unobserved, but $\hat{y}_i$ is estimated from the regressions in Section III, where $\hat{y}_i = y_i + v_i$, $E(v_i) = 0$, $E(v_i^2) = \sigma_i^2$, and ε_i and v_i are assumed to be independent. The heteroscedasticity arises because the estimated regressions are given by

$$\hat{y}_i = Z_i\beta + (\varepsilon_i - v_i) = Z_i\beta + \mu_i,$$

where $E(\mu_i) = 0$ and $E(\mu_i^2) = \sigma_\varepsilon^2 + \sigma_i^2$. The OLS regression of the second stage provides an estimate of $\hat{\mu}_i$, and combined with the estimates of $\hat{\sigma}_i^2$ available from the first-stage regressions, the parameter σ_ε^2 can be estimated by

$$\hat{\sigma}_\varepsilon^2 = \left(SSE - \sum \hat{\sigma}_i^2\right)/(N-K),$$

where SSE is the error sum of squares from the OLS second-stage regression and $N-K$ is the number of degrees of freedom. The calculated $\hat{\sigma}_\varepsilon^2$ is then used to reestimate the second-stage regression using generalized least squares.

[22] These results are consistent with the estimated gains to English language proficiency reported in Walter McManus et al. (1983).

TABLE 5—DETERMINANTS OF THE ENTRY WAGE DIFFERENTIAL BETWEEN THE 1979 IMMIGRANT COHORT AND NATIVES[a]

Country of Origin Characteristics	Regression 1	2	3	4
Intercept	−.2214	.1838	−.9934	−.9469
	(−3.88)	(1.06)	(−3.41)	(−3.30)
Politically Competitive System	.2743	.1306	.1101	.1264
	(4.49)	(2.01)	(2.16)	(2.39)
Recent Loss of Freedom	−.0010	−.0511	−.0062	.0136
	(−.01)	(−.75)	(−.12)	(.25)
Number of Assassinations	−.0072	−.0028	.0021	.0044
	(−1.20)	(−.54)	(.51)	(.92)
Income Inequality	−.0084	−.0038	.0039	.0046
	(−1.78)	(−.89)	(1.02)	(1.13)
Distance from U.S.	–	−.0114	−.0031	.0018
		(−.89)	(−.31)	(.09)
English Proficiency	–	.2596	.1980	.2030
		(2.20)	(2.12)	(2.21)
Mean Age at Migration	–	−.0217	−.0149	−.0119
		(−3.55)	(−2.99)	(2.28)
ln (per capita *GNP*)	–	–	.1164	.1015
			(4.57)	(3.77)
Country in Asia or Africa	–	–	–	−.1145
				(−1.58)
Country in North or South America	–	–	–	−.0640
				(−.73)
R^2	.504	.681	.808	.826

[a] The t-ratios are presented in parentheses.

immigrants by about 1.2 percent. This effect is likely to be caused by the fact that the higher the GNP in the country of origin, the greater the resemblance between that country's economic structure and that of the United States, as well as the greater the skills of the immigrant flow. Hence immigrants from those countries perform quite well in the U.S. labor market. The last regression in Table 6 adds continent dummies (the omitted continent is Europe) to control for continent-specific fixed effects. These additional controls do not have a major impact on the coefficient of the other variables.[23]

Table 5 shows that controlling for variations in per capita *GNP* across countries of origin has a major impact on the coefficient of the inequality variable: the latter turns positive (and insignificant). The reason for this shift lies in the very high negative correlation between the two variables ($r = -.6$). Since high-income countries (mostly in Western Europe) also tend to have the least amount of income inequality, the impact of per capita GNP on initial immigrant quality is likely capturing shifts in both the mean and the variance of the country of origin's income distribution.

The results in Table 5, therefore, are not entirely consistent with the theoretical predictions. Note, however, that these regressions do not truly constitute a "test" of the theory. The Roy model shows that selection biases will depend on a number of parameters which are not directly measurable. Table 5 attempts to explain intercountry differences in terms of variables which sup-

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[23] To further test the sensitivity of the results, two additional variables were introduced into the regression: the percent of the country's labor force that is in agriculture, and the per capita school enrollment rate. Both of these variables were highly correlated with GNP per capita, and in fact became insignificant once GNP was controlled for. Their impact on the other variables in the regression was negligible.

TABLE 6—DETERMINANTS OF THE RATE OF ASSIMILATION[a]

Country of Origin Characteristics	Regression 1	2	3	4
Intercept	.0076	−.0240	−.0237	−.0280
	(2.96)	(−3.88)	(−1.50)	(−2.32)
Politically Competitive System	−.0029	−.0068	−.0068	−.0091
	(−1.06)	(−2.66)	(−2.60)	(−4.28)
Recent Loss of Freedom	.0063	.0029	.0030	.0021
	(1.81)	(1.21)	(1.15)	(1.06)
Number of Assassinations	.0008	.0006	.0006	.0008
	(2.68)	(2.36)	(2.14)	(3.07)
Income Inequality	−.0001	−.00002	−.00002	.0002
	(−.50)	(−.11)	(−.10)	(.90)
Distance from U.S.	–	.0003	.0003	−.0027
		(.74)	(.70)	(−2.89)
English Proficiency	–	.0138	.0138	.0122
		(3.27)	(3.20)	(3.70)
Mean Age at Migration	–	.0009	.0009	.0009
		(4.28)	(3.95)	(4.72)
ln (per capita *GNP*)	–	–	−.00002	.0021
			(−.01)	(1.83)
Country in Asia or Africa	–	–	–	.0151
				(5.11)
Country in North or South America	–	–	–	−.0080
				(−2.08)
R^2	.302	.704	.704	.842

[a] The t-ratios are presented in parentheses.

posedly proxy for these primitive concepts. Clearly the errors introduced in the creation of these variables weaken the link between the theory and the empirical work. Nevertheless, it is important to note that these few country-specific variables "explain" a large fraction of the intercountry differences evident in census data.

B. *Determinants of the Rate of Assimilation*

The assimilation rate is defined by the rate of earnings growth of an immigrant cohort (relative to natives) evaluated at ten years after immigration. Table 6 presents the regressions of this variable on the various country-specific proxies.

Assimilation rates are determined by political factors. In particular, immigrants from free countries have lower assimilation rates than immigrants from countries with a long history of political repression, while immigrants from countries that recently lost their political freedom have the highest assimilation rates. These results are consistent with the hypothesis that the costs of return migration for immigrants from politically repressive countries are high, and therefore they have the most incentives to adapt to the U.S. labor market. The same reasoning can also explain the strong positive impact of the number of assassinations on the rate of immigrant assimilation: immigrants from politically unstable countries have greater incentives to assimilate in the U.S. labor market since their return migration may be costly.

The regression in column 2 shows that although distance between the United States and the country of birth has a positive impact on the assimilation rate, the effect is not significant. However, immigrants from countries with higher levels of English proficiency have much higher assimilation rates. In fact, the rate of earnings growth of immigrants from English-speaking countries is 1.4 percentage points higher than that of im-

TABLE 7—DETERMINANTS OF THE RATE OF CHANGE IN COHORT QUALITY

Country of Origin Characteristics	Regression 1	Regression 2	Regression 3	Regression 4
Intercept	−.3194	−.9951	−1.1779	−2.2202
	(−3.19)	(−3.97)	(−4.08)	(−4.69)
Politically Competitive System	.1760	.1075	.0712	.0630
	(2.54)	(1.60)	(.97)	(.70)
Recent Loss of Freedom	.1256	.1468	.1272	.1310
	(1.67)	(2.16)	(1.81)	(1.33)
Number of Assassinations	.0077	.0156	.0122	.0256
	(1.19)	(2.32)	(1.69)	(2.00)
Rate of Change in Central Government Expenditures	.0698	.0699	.0641	−.0099
	(1.60)	(1.75)	(1.60)	(−.21)
Rate of Change in Per Capita GNP	4.7010	3.0956	1.1567	−1.5321
	(2.27)	(1.60)	(.46)	(−.50)
ln (per capita *GNP*)	–	.0889	.1186	.2443
		(1.93)	(3.22)	(4.15)
Country in Asia or Africa	–	–	.1374	–
			(1.42)	
Country in North or South America	–	–	.0274	–
			(.41)	
Change in Quota	–	–	–	.0034
				(2.26)
R^2	.284	.418	.453	.581

[a] The *t*-ratios are presented in parentheses.

migrants from countries with 0 percent English proficiency rates. Similarly, the age at immigration has a strong *positive* impact on assimilation rates. This result is consistent with the hypothesis that immigrants who migrate as youths have little to gain from assimilation per se. On the other hand, the adaptation period is likely to be important for persons who migrate at older ages.

The last two regressions in Table 6 include per capita *GNP* variable and the continent dummies in the list of regressors. These variables have a significant impact on the assimilation rate (in column 4). Immigrants from wealthier countries have higher assimilation rates, and Europeans (the omitted continent) have higher assimilation rates than immigrants from the Americas, but lower assimilation rates than immigrants born in Asia or Africa. Despite the strongly significant impact of the continent dummies, the qualitative effect of most of the other variables in the regression is unaffected.

C. *Determinants of the Change in Cohort Quality*

Section III calculated a variable measuring the wage differential between the 1979 immigrant cohort and the 1955 immigrant cohort as of the date of immigration. The regressions analyzing the determinants of cohort quality change are presented in Table 7. It should be noted that the specification of these regressions differs slightly from those presented in Tables 5 and 6 since cohort quality change is likely to be determined by *changes* in the explanatory variables over the 1954–79 period.

The simple specification in column 1 reveals that cohort quality change is strongly influenced by practically all the variables in the regressions. For example, the quality of cohorts from countries that experienced a shift from political competition to repression increased by about 13 percent (relative to the quality of cohorts from countries that were politically repressive throughout the period). This effect is consistent with the implications of the theory developed in Section I. The change in political structure can be viewed as a change in the correlation coefficient of the earnings of individuals between the home country and the United States. The change toward a repressive government may make the correlation coefficient in earnings across the two countries negative. Thus persons in the "revolutionary" lower tail of the home country's income distribution migrate to the United States and perform quite well in the U.S. labor market.

Table 7 also shows that cohort quality change is strongly affected by the average annual change in the percent of GNP that is attributable to expenditures by the central government. Presumably the greater the role of the government, the more income redistribution that takes place and the greater the *decrease* in income inequality over the postwar period. The coefficient of this variable in Table 8 is consistent with the theoretical implication.

The next two regressions in Table 8 add the level of per capita GNP to control for country-specific differences in wealth, and the continent dummies to control for continent-specific fixed effects. The continent dummies are not very significant, but per capita GNP variable does have a strong positive impact on cohort-quality change. Its positive coefficient confirms the finding suggested by the descriptive analysis in Section III: the quality of immigrants admitted to the United States has been increasing over time when the immigrants originate in Western Europe and has been declining over time when the immigrants originate in the less developed countries.

TABLE 8—PROBIT REGRESSION ON THE EMIGRATION RATE[a]

Country of Origin Characteristics	Regression 1	Regression 2
Intercept	−.6060	−1.1614
	(−1.30)	(−2.46)
Politically Competitive System	.1206	.0801
	(1.13)	(.81)
Recent Loss of Freedom	.1096	−.0365
	(.95)	(−.32)
Number of Assassinations	−.0245	−.0337
	(−2.65)	(−3.65)
Income Inequality	−.0113	−.0145
	(−1.51)	(−2.00)
Distance from U.S.	−.1332	−.1271
	(−6.11)	(−2.68)
English Proficiency	.1661	.0488
	(.94)	(.30)
ln (per capita *GNP*)	−.1130	−.0441
	(−2.14)	(−.83)
Country in Asia or Africa	–	.3386
		(2.19)
Country in North or South America	–	.2923
		(1.52)
χ^2	98.45	108.82

[a] The dependent variable is the probability that an individual migrated to the United States in 1951–80, and is given by the second column of Table 2. The *t*-ratios are presented in parentheses.

One factor causing systematic quality shifts across immigrant cohorts may have been the change in the quota system mandated by the 1965 Amendments to the Immigration and Nationality Act. Table 4 defined a variable that measures the change in the fraction of the home country's population "eligible" for emigration to the United States before and after the 1965 Amendments. Higher levels of this variable imply a reduction in the levels of "mobility costs" faced by potential emigrants. The Roy model suggests that its impact on the rate of cohort-quality change is positive if the correlation coefficient between earnings capacities in the two countries is positive and if income is more unequally distributed in the countries of origin than in the United States. The last column of Table 7 adds the quota variable to the regression and shows that it indeed has a positive and significant impact on the rate of change in cohort quality. It is important to note that this regression is estimated on only 28 observations since the quota system prior to the 1965 Amendments was applicable only to countries that were in the Eastern Hemisphere.

D. *Determinants of the Emigration Rate*

The empirical analysis in this paper focuses on the determinants of the (relative) earnings of immigrants. It is worth noting, however, that the Roy model also implies that the emigration rate will be a function of the same characteristics of the income distribution, political conditions, and migration costs that determine the relative earnings of immigrants. Therefore, it is important to explore if the emigration rate from the various countries of origin is responsive to shifts in the country-specific variables that have been used throughout this section.

Table 8 presents two probit regressions on the emigration rate.[24] The dependent variable is obtained from the second column of the summary statistics presented in Table 2, and is the fraction of the country of origin's population that emigrated to the United States in the 1951–80 period. The first of the two regressions includes the political variables, the distance variable (to measure migration costs), and the inequality variable, while the second regression adds the continent dummies.[25]

As expected, the distance between the country of origin and the United States has a negative impact on the emigration rate. The emigration rate is also lower for countries that have high levels of GNP per capita. These results, of course, are consistent with the predictions of the wealth-maximization framework. More interestingly, the second moment of the income distribution (as predicted by the Roy model) plays an important role in the determination of the emigration rate. In particular, countries with more income inequality have lower emigration rates. This negative coefficient is implied by the wealth-maximization framework if there is negative selection in the immigrant pool. Negative selection requires that the correlation between earnings in the United States and in the sending countries be sufficiently positive *and* that the United States has less income inequality than the sending countries. If, in addition, mean income in the United States exceeds mean income in sending countries (adjusted for mobility costs), as income inequality in the home country increases, the migration incentives of the most able decreases while the poorest will still migrate. Hence the emigration rate *declines* due to the withdrawal of high-income persons from the pool of emigrants. The analysis of the emigration rate, therefore, leads to results that are generally consistent with the types of selection biases that have been documented in this paper.

V. Summary

What determines the (labor market) quality of foreign-born persons in the United States? Most of the literature addresses this question simply by assuming that immigrants are a "select" group, and that the selection mechanism somehow sends the most able and the most ambitious persons in any country of origin to the United States. This paper is an attempt to analyze both the conceptual and empirical foundations for this type of assertion. Among the major findings of the study are:

1) If potential emigrants are income maximizers, foreign-born persons in the United States need not be drawn from the most able and most ambitious in the country of origin. Two conditions must be satisfied in order for positive selection to take place: a) there is a strong positive correlation between the earnings a worker may expect in the home country and the earnings the same worker may expect in the United States; and

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[24] The probits were estimated using generalized least squares. The estimator, therefore, is minimum *chi*-squared and efficient.

[25] The regressions in Table 8 exclude the age at migration from the list of regressors since this variable was calculated in the subsample of immigrants and may have little relationship to the age distribution of the population in the country of origin.

b) the United States has a more unequal income distribution than the home country. If the income distribution in the sending country is more unequal than that of the United States (and the correlation in earnings is positive and strong), emigrants will be chosen from the *lower* tail of the income distribution in the country of origin.

2) The empirical analysis of the earnings of immigrants from 41 different countries using the 1970 and 1980 censuses shows that there are strong country-specific fixed effects in the (labor market) quality of foreign-born persons. In particular, persons from Western European countries do quite well in the United States, and their cohorts have exhibited a general *increase* in earnings (relative to their measured skills) over the postwar period. On the other hand, persons from less developed countries do not perform well in the U.S. labor market and their cohorts have exhibited a general *decrease* in earnings (relative to their measured skills) over the postwar period.

3) The empirical analysis of the variance in various dimensions of immigrant incomes shows that a few variables describing political and economic conditions in the various countries of origin explain over two-thirds of the intercountry variance in the mean U.S. incomes of immigrants with the same measured skills. Immigrants with high incomes in the United States relative to their measured skills come from countries that have high levels of GNP, low levels of income inequality, and politically competitive systems.

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Measuring the Value of a Public Good: An Empirical Comparison of Elicitation Procedures

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The practical problems associated with accurately measuring the value of a public good in an applied setting are considered. We compare and contrast the values obtained from hypothetical elicitation procedures with those obtained in a marketplace. When hypothetical measurements are elicited in the field, buying-selling discrepancies similar to those predicted by psychological models of behavior are observed. These discrepancies decrease greatly when a market with appropriate incentives for accurate relevation is used to elicit the value for the public good.

A large body of recent research has been concerned with the psychological phenomenon variously labeled the endowment effect (Richard Thaler, 1980), reluctance to trade (Daniel Kahneman and Amos Tversky, 1984), and the buying-selling price discrepancy (Jack Knetsch and John Sinden, 1984; and Ronald Cummings, Brookshire, and William Schulze, 1986). According to the results of this research, people require far more compensation to give up a public good than they are willing to pay to acquire it. The empirical differences in these two measures are much larger than can be accounted for by any income effects which are derived from a utility-based model. Following received economic theory (Robert Willig, 1976; Alan Randall and John Stoll, 1980; and Akira Takayama, 1982), there should be symmetry in value measurements when the quantity of the public good is increased or is decreased away from a baseline level, especially when the quantities in question are relatively small.

The existence and stability of this psychological phenomenon has been challenged by another body of literature. In a series of studies, researchers have concluded that the initial discrepancy between compensation demanded and willingness to pay is an anomaly, which can properly be eliminated by the forces of learning and incentives which are present in a market (Peter Knez et al., 1985; Coursey et al., 1987). We extend these results by considering in this paper the practical problems associated with accurately measuring the value of a public good in an applied setting. Specifically, we compare and contrast the values obtained from hypothetical elicitation procedures with those obtained in a marketplace. We find that when hypothetical measurements are elicited in the field, buying-selling discrepancies similar to those predicted by psychological models of behavior are observed. These discrepancies decrease greatly when a market with appropriate incentives for accurate revelation is used to elicit the value for the public good.

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I. A Framework for Comparing Public Good Valuation Measurements

Assume that there are $N+1$ goods over which an individual consumer has a preference ordering. The first N of these are represented by a vector $x=(x_i : i \in N)$ while the $N+1$st commodity, the public good, is denoted by a scalar z. We assume that the goods which make up x are traded in markets at strictly positive prices $p=(p_i : i \in N)$ over which the consumer has no influence. The public good z is not traded in a market, but is determined by the level of H attributes, $a=(a_h : h \in H)$. Each of these attributes has a price q_h. These prices depend upon the level of attributes associated with the commodities, and so will implicitly reveal a function $q(a)$ relating prices and attributes in Sherwin Rosen's (1974) sense of hedonic prices.

The consumer's preferences are described by the strictly increasing, quasi-concave, ordinal utility function $U(x,a)$ The budget constraint which the individual faces is $p \cdot x + q(a)z \leq y$, where $y>0$ is the consumer's exogenous income. In this case, we may write the consumer's indirect utility function as[1]

$$(1) \quad V(p,q(a),y) = \max_{x,a} \{U(x,a) : p \cdot x + q(a)z \leq y\}.$$

Consider the Hicksian income compensation function for two situations where only the level of the public good's attribute is changed from a^0 to a'. The compensation demanded for a decrease $(a' < a^0)$ in the public good's attributes is defined by a willingness-to-accept (WTA) function:

$$(2) \quad \mathrm{WTA}(p^0,q(a');\ p^0,q(a^0),y^0) = \min_{y} \{y>0 : V(p^0,q(a'),y) \geq V(p^0,q(a^0),y^0)\}.$$

Similarly, the willingness to pay for an increase $(a' > a^0)$ in the public good's attributes is defined by a willingness-to-pay (WTP) function:[2]

$$(3) \quad \mathrm{WTP}(p^0,q(a');\ p^0,q(a^0),y^0) = \max_{y} \{y>0 : V(p^0,q(a'),y) \geq V(p^0,q(a^0),y^0)\}.$$

When, however, small decreases in the public good's attributes described in (2) are compared to similar increases in the public good's attributes described in (3) then, as discussed in the introduction, empirical value measurements are found to depend upon the *direction* of the attributes' change.[3]

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[1] In this analysis, as well as in the conduct of the empirical work to follow, we are assuming that the attributes are use-related variables as opposed to existence values. The attributes enter the utility function only by making the public good more desirable for consumption. As will become apparent shortly, we will treat these attributes as if they themselves were public goods, following the approach of the other field studies associated with visibility, water quality, and other so-called public goods. Olvar Bergland and Randall (1984) provide an outline of the theoretical restrictions which this assumption about attributes imposes upon preferences.

[2] Bergland and Randall provide a derivation for obtaining the correct calculation of these Hicksian variations from observable data.

[3] In an unpublished paper, Michael Hanemann (undated) has shown that holding income effects constant, greater disparity between WTP and WTA is implied by the availability of fewer substitutes for the public good. Thus if there are private goods which can be readily substituted for z, then there ought to be little difference between an individual's WTP and WTA for a change in z. The assumption of a large degree of substitutability is implicit in our empirical research. As will be seen, the attribute under examination in this study is trees, which have both intra- and interpark substitutes readily available. An individual is assumed to possess a utility function which allows trees to be traded off against other park attributes and against other completely different parks. In the actual collection of individual bids which is described in the next section, it is assumed that the medium which allows these tradeoffs to take place is money or a Hicksian composite commodity. Even if one were to reject the conjecture that money or a Hicksian composite commodity allows these tradeoffs to take place, it is doubtful Hanemann's argument could justify the extremely large differences between willingness to pay and willingness to accept that we observe in our field studies.

Economic speculation about the nature of this asymmetry has centered upon the strategic biases associated with different elicitation procedures. The conventional wisdom emphasizes that the provision of a public good is nonoptimal when individual values are solicited outside of a market or "market-like" context. If respondents treat a public good evaluation procedure as a nonmarket process, a logical strategy is to exaggerate their actual values. In a willingness-to-pay environment, they will tend to respond with value measures which understate their actual demand for the public good. Likewise, in a willingness-to-accept environment, they will tend to overstate the compensation required to reduce the level of a public good's provision. Without the addition of a market-like elicitation procedure that induces truthful revelation of value, the gap and associated asymmetry between willingness-to-pay and willingness-to-accept measures is not expected to disappear.

Psychologists attribute the asymmetry in value measurements to another problem. Their central argument hinges upon the conjecture that an individual may not exhibit a coherent set of preferences. Specifically, the prospect theory of behavior includes an analysis of value that compares different levels of a good's provision to a neutral frame of reference point. This reference point is usually taken to be the current level of provision of the good. The most important implication of this model is that decreases in the quantity of the good away from this reference point are valued at a marginally higher rate than corresponding increases in the quantity of the good above the reference point. This phenomenon of losses looming larger than gains is usually referred to as "loss aversion." The main implication of loss aversion is that when equal increases and decreases in quantity are valued, one should not expect to observe symmetry between willingness-to-pay and willingness-to-accept measures.

Although the explanations of asymmetry differ across the two professions, two common features prevail. First, both argue that it is impossible to separate the measured value of the public good from the elicitation procedure by which that value is obtained. More specifically, the economist may argue that any biases in measured value over different elicitation techniques are a direct reflection of the strategic incentives associated with these techniques. The psychologist may place more argumentative emphasis upon the notation that biases arise from variations in the framework through which values are elicited.

The second common feature relates to the joint professional belief that the value asymmetry will disappear in a market-like environment. A market-like environment will accomplish the role of providing both incentives to accurately state demand and learning opportunities to the individual. The first important attribute of the market-like environment is the incentive property; specifically, the design of a public good allocation process which provides the greatest possible incentive for truthful revelation of value. Once truthful values are obtained, any sources of bias which lead to the asymmetry should then disappear. Additionally, the market-like environment is important because recurrent and reversible transactions can take place in a market. The importance of these transactions lies in the fact that attitudes toward losses may change as the individual becomes familiar with the experience of obtaining a public good and then giving it up. After a period of time, what is given up will be perceived as an opportunity cost, rather than a loss. The loss-aversion phenomenon can then be expected to become a less predominant factor in the valuation measurement process.[4]

Whichever explanation is invoked to explain asymmetry, it is apparent there will be differences when attribute levels are either decreased or increased. The important empirical issue is whether this asymmetry will disappear based upon arguments that provide for correct incentives and repeated learning experiences.

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[4] See Kahneman's comments in Cummings et al. for background on the dynamics of how individuals exhibit less loss aversion through the trading process. Psychologists also argue that other factors such as bidding low or requesting large compensation in situations where preferences are unknown may help explain the loss-aversion phenomenon. These sources of the disparity are also predicted to disappear as individuals "learn" their values in the marketplace.

Hypothesis 1: *Will a hypothetical, nonmarket elicitation process for valuing a public good in an applied field setting yield symmetric baseline valuation measurements?*

This hypothesis is tested through the use of a survey which directly elicits values in the field. Survey or contingent valuation methods are distinguished from other forms of public good valuation methods by their use of a questionnaire to acquire information. Central to this approach is the construction of a hypothetical allocation and valuation procedure for the good. The procedure is conducted as follows. First, the public good under study is described to the individual. Second, the hypothetical situation involving either a decrease or increase in the quantity of the public good is posited to the individual. Finally, each individual responds with a compensation-demanded or willingness-to-pay value.

Hypothesis 2: *Does the addition of the primary demand-revealing properties of an incentive-compatible public good allocation mechanism in the field result in elimination of the asymmetry between valuation measurements?*

In a series of papers Vernon Smith (1977; 1979a, b; 1980), John Ferejohn, Robert Forsythe, and Roger Noll (1979a, b), Ferejohn, et al. (1982), and Martin Loeb (1977) have considered the problem of designing a market-like auction process to elicit the value of a public good. This process involves designing a process based upon a Theodore Groves-John Ledyard (1977) mechanism for providing a public good.

In experimental examinations of such a public good auction mechanism (for example, Smith, 1979a, b; 1980, and Coursey-Smith, 1984) using induced values (see Smith, 1976, 1982), Lindahl optimal quantities of a public good were provided by groups with up to nine members. These studies, and other studies conducted in the field by Peter Bohm (1972), Ferejohn and Noll (1976), and Bruce Scherr and Emerson Babb (1975), suggest that it is possible to elicit truthful, or at least more accurate, values for a public good using the auction mechanism.

Hypothesis 3: *Are the additions of repetition and actual monetary consequences in a controlled interactive laboratory setting required to eliminate the asymmetry in valuation measurements?*

An important additional conclusion to be drawn from the public good allocation studies mentioned above is that individuals require a period of time and interaction to "learn" that their optimal strategy is demand revelation. A public good auction process is necessary for demand revelation, but not always sufficient unless combined with a period of time over which experience and learning can occur. In testing Hypothesis 3, we allow for iterated interactions to take place in a controlled laboratory environment using a market-like mechanism. This is accomplished through the use of an equilibration process in which individuals can acquire a quantity of the public good z using an allocation process which determines a market-like value of $q(a)$. Additionally, as will be explained in the next section, individuals were made to realize actual (as opposed to hypothetical) monetary consequences of their actions.

II. Data Collection Methodology

A. *The Public Good*

To operationalize the hedonic compensation-demanded and willingness-to-pay measures described by (2) and (3), we concentrated our value elicitation techniques upon one attribute of a neighborhood park in Fort Collins, Colorado. To capture the public good nature associated with one attribute, we focused our attention upon alternative densities of trees in Troutman Park, a new, undeveloped recreation area in the city.

In the development of 25 parks in the past, Fort Collins has planted an average of

10 trees per acre. Applying this average to the 20-acre Troutman Park, 200 trees were assigned as a frame of reference point from which to establish alternative attribute increments for the compensation demanded and willingness-to-pay value measurements. The City Parks and Recreation Department was consulted to establish what would be discernible yet small levels of alternative tree densities upon which to base our value comparisons. Through a process of pretesting, it was established that increases and decreases over two increments, ±25 and ±50 trees, were visually and aesthetically distinguishable when an artistic representation of the park was utilized. This range of tree densities represents the historical range of densities found within the other 25 parks in the Fort Collins area.

A portfolio developed by a professional artist provided visual representations of different tree densities in the park from three vantage points.[5] These vantage points illustrated how the trees would look in the park from three alternative views. The complete portfolio consisted of a 3×3 matrix of illustrations. Each column of the portfolio presented a different perspective of the park. Column A presented a perspective of the park from the northeast corner looking toward the mountains. Column B provided a bird's-eye view of the park indicating the overall density of trees. Finally, column C illustrated a perspective of the park from the southwest corner looking toward town. The rows of the portfolio presented the alternative density levels of the trees with each tree depicted at approximately 75 percent maturity. Separate portfolios were utilized for the willingness-to-pay and willingness-to-accept questions where, for each case, the frame of reference level of trees was 200. For the willingness-to-pay questions, the density levels by row were 200 trees (row 1), 225 trees (row 2), and 250 trees (row 3). For the willingness-to-accept questions, the increments represented by the rows were 200 trees (row 3), 175 trees (row 2), and 150 trees (row 1).

B. *Sample Plan*

Three independently conducted elicitation procedures were used to collect valuation measurements. A hypothetical contingent valuation methodology (CVM) was administered door-to-door. A Smith auction process, which is based upon Smith's (1980) decentralized mechanism for allocating public goods, provided the framework for the second hypothetical elicitation technique used in the field (SAF). Last, laboratory versions of the Smith auction (SAL) process were conducted in a local high school. For all three elicitation techniques, the presentation of the alternative tree levels used the portfolios and was identical in verbal and graphical content. In all three cases, additional information pertaining to uses of parks, preferences over park attributes, and socioeconomic data were also collected.

A one-square-mile area around Troutman Park, bordered by four major streets, defined the sample area for the CVM, SAF, and SAL valuation exercises. This square-mile area contains 667 houses. To insure that each elicitation technique would have observations in all regions of the sample area, the one-square-mile area was further divided into three sectors of equal distance from Troutman Park. Linear distance to the park, street distance to the park, and view of the park were used as guidelines so that each sector had approximately the same numbers of houses. The houses in each sector were numbered and then assigned randomly to one of three elicitation procedures. Two interview teams were used to complete the surveys. Within each sector and over each elicitation procedure, these teams were assigned in a random fashion separate from the initial household randomization.

C. *The Data Collection Methods*

1. *Contingent Valuation Methodology* (CVM). The procedures for obtaining CVM valuation measures followed traditional techniques as outlined by Cummings et al. and

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[5] A representation of the portfolio, the survey instruments, and the experimental instructions are available upon request from the authors.

Icek Ajzen and Martin Fishbein (1977). After the alternative tree density levels were explained using the visual portfolios, it was proposed that the city would pay (collect) a one-time payment to (from) households to change the base plan of 200 trees. A list of possible dollar value payments was used by the household to determine their willingness-to-accept (willingness-to-pay) response. The specific question asked was: "*What minimum dollar amount would your household be willing to accept to decrease the base plan to 175 trees under this one-time payment?*" The question was then repeated for a decrease from 200 to 150 trees. The willingness-to-pay question was: "*What maximum dollar amount would your household be willing to contribute to a fund to increase the base plan from 200 to 225 trees under this one-time payment?*" The question was then repeated for an increase from 200 to 250 trees. If in any case the response was zero, the individual was asked why.

2. *The Field Smith Auction Process* (SAF). The second field study combined elements of Smith's public good auction process with traditional CVM techniques. Although an auction was not conducted, the context of the questions asked reflected the important elements of this process. The CVM procedure outlined previously does not ask the individual to consider the valuation question in the context of what other individuals are bidding. Additionally, the CVM procedure does not present information to the household pertaining to the cost of the alternative tree density levels. Both of these elements are included by necessity in a Smith auction procedure.

The structure of the willingness-to-pay Smith auction procedure used in the field was as follows. The household was asked to assume that they were participating in the valuation process with the other households in the community. It was stated that offers from each of the 667 households in the immediate Troutman Park neighborhood were being collected to pay for the two alternative expansions to 225 and 250 trees.

The interview team explained that there were three possible outcomes depending on the sum of the payments from the households. First, if the sum of the payments was less than the cost of the additional trees, the base plan would not be expanded and the household would pay nothing. Second, if the sum of the payments equaled the cost of the additional trees, the base plan would be expanded and the household would pay exactly the amount it had offered. Third, if the sum of the payments was greater than the cost of the additional trees, the base plan would also be expanded. However, the household would not have to pay the full amount it offered. The offers in this case would be proportionally scaled back so that their sum exactly equaled the cost of the trees. Numerical examples of all three cases were provided to the respondent.

After stating the total cost of expanding the base plan from 200 to 225 trees, the household was asked: "*What maximum dollar amount would your household be willing to contribute to a fund to implement this expansion?*" The total cost for an expansion from 200 to 250 trees was then provided to the respondent and the willingness-to-pay question was asked again over the new increment. If the household was unwilling to contribute on either situation, they were asked why.

The willingness-to-accept question asked in the SAF surveys was a mirror image of the above. Each household was told that it could receive reimbursement from the group fund of fixed size in order to compensate for a reduction from the base level of 200 trees to 175 and 150 trees. Again, three outcomes were possible. First, if the sum of the requests was greater than the amount of the fund, the base plan would be followed and no reimbursement would be paid. Second, if the sum of the requests equaled the fund, the base plan would be reduced and the household would be reimbursed by exactly the amount requested. Third, if the sum of the requests was less than the fund, the base plan would be reduced but the household would receive more compensation than requested. The requests would be proportionally increased so that their sum exactly equaled the fund. Examples of the three possible outcomes were provided to the households, and care again was taken to insure a complete understanding of the process.

After stating the total fund that would be available if the base plan was reduced from 200 to 175 trees, the household was asked: "*What minimum dollar amount would your household be willing to request from the group fund to implement this reduction?*" Then the total fund for a reduction of the base plan to 150 trees was provided to the respondent and the minimum compensation question was repeated.

3. *Laboratory Smith Auctions* (SAL). While the SAF procedure is designed to capture the incentive elements of a Smith auction process, it fails short in its ability to allow for the more complete revelation of responses that can occur in a repetitive market-like environment. Additionally, the SAF procedure is still rooted in hypothetical transactions, for which respondents never realize actual monetary consequences.

Fundamentally, the process of collecting payments for the group fund or requesting compensation from the group fund used in the SAF procedures was the same as that used in the laboratory. The groups consisted of individuals who participated in experimental sessions at the local high school. The fund consisted of money that was actually collected or distributed from or to the group. The amount of money in the fund was determined by using actual per household costs of either increasing or decreasing the tree density levels. As with the SAF procedures, these fund levels were known to the participants.

However, instead of eliciting payments or compensation only once from the participants, five possible trials (iterations) of the Smith auction process were possible. During each of these trials, individuals privately submitted willingness-to-pay or willingness-to-accept bids. These bids were then summed by the experimental monitor and this sum was reported to the group. As in the SAF procedures, three cases were possible: the bids exceeded, were equal to, or were less than the group fund level in question. If the group covered the cost of an expansion of trees after the second trial in the willingness-to-pay experiments, then that experiment concluded. In this case, the individuals would actually have to pay their (possibly adjusted) stated bids. This money was then contributed to Fort Collins' Recreation Department. If the group requested an amount of money less than or equal to the funds freed by a reduction of trees in the willingness-to-accept experiments, then that experiment ended. In this case, the individuals would actually be compensated their (possibly adjusted) bids from a fund of cash that otherwise would have been contributed to Fort Collins' Parks and Recreation Department.[6] In the other cases when the group either did not cover the cost of an expansion or requested more compensation than available for a reduction, another trial was conducted. Individuals then had the opportunity to change their bids.

If the group did not contribute an appropriate amount to or request an appropriate amount from the fund by the fifth trial, then the experiment ended automatically. When the willingness-to-pay experiments ended this way, no contribution for trees was made; when the willingness-to-accept experiments ended this way, no compensation was provided to the participants. It is this repetitive feature, combined with the fact that individuals only pay their adjusted bids, not necessarily what they bid, which makes the Smith auction perform as well as it does.[7,8]

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[6] A sum of $1,500 was ultimately contributed to Fort Collins' Parks and Recreation Department for use in purchasing trees for Troutman Park.

[7] As noted by Ferejohn et al., the Smith auction process may not be expected to eliminate all underbidding in the willingness-to-pay experiments or all over requesting in the willingness-to-accept experiments. However, empirical evidence from induced valuation experiments conducted by a variety of researchers has indicated that the Smith auction can be expected to yield at least 80 percent efficient outcomes in most cases. When the same individuals in these experiments use similar induced valuation designs which do not utilize the Smith auction, efficiency measures of performance are found to drop significantly.

TABLE 1—SUMMARY STATISTICS: WILLINGNESS-TO-PAY AND WILLINGNESS-TO-ACCEPT BIDS

	Field Surveys							
	CVM-WTP		CVM-WTA		SAF-WTP		SAF-WTA	
	25[a]	50[b]	25[c]	50[d]	25[a]	50[b]	25[c]	50[d]
$\bar{x}$[e]	14.00	19.40	855.50	1734.40	14.40	15.40	807.20	1735.00
Median	9.60	9.30	199.80	399.30	11.80	13.80	30.30	100.40
σ	18.40	28.20	1893.20	3775.80	12.40	15.30	2308.00	4391.10
Number of Observations	48	48	45	45	47	47	47	47
	Laboratory Experiments							
	Initial Bids SAL-WTP		Final Bids SAL-WTP		Initial Bids SAL-WTA		Final Bids SAL-WTA	
	25[a]	50[b]	25[a]	50[b]	25[c]	50[d]	25[c]	50[d]
$\bar{x}$[e]	7.31	8.33	7.31	12.92	28.63	67.27	17.68	95.52
Median	9.33	2.50	5.09	7.50	15.00	20.00	7.25	18.66
σ	6.39	10.08	6.52	14.38	26.48	132.02	23.85	272.08
Number of Observations	17	12	17	12	12	13	12	13

[a] WTP of respondent for an increase in the base plan from 200 to 225 trees.
[b] WTP of respondent for an increase in the base plan from 200 to 250 trees.
[c] WTA compensation of the respondent for a decrease in the base plan from 200 to 175 trees.
[d] WTA compensation of the respondent for a decrease in the base plan from 200 to 150 trees.
[e] All bids are denoted in dollars.

Fifteen dollars was provided to all experimental participants to insure their arrival at the high school. In the willingness-to-pay experiments, individuals were provided an additional endowment of funds. Fifteen dollars was provided in the 25-tree increment experiments and $30 was provided in the 50-tree increment experiments. No individuals declined to participate in the experiments, but they could have kept their total endowments if they had declined.[9]

III. Comparisons of Results

Table 1 presents summary data for the three elicitation techniques.[10] Our data suggest that compensation demanded for a tree reduction is significantly larger than willingness to pay for a similar-sized expan-

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[8] A technical addition was added to the design which allowed the participants to learn about the auction process itself, as opposed to learning about incentives. The first two trials were nonbinding in the sense that even if the group contributed or requested a plausible amount of money from the fund, the experiment would not end. This has been shown by Coursey-Smith and Coursey et al. to accelerate the learning process. See additionally Jeffrey Banks, Charles Plott, and David Porter (1986) for a discussion of various operational versions of the Smith auction process.

[9] The endowments provided in the willingness-to-pay experiments may invoke a small income effect. However, if the endowments provided are similar to the compensation which individuals receive in the willingness-to-accept experiments, it can be shown that the income effects in both sets of experiments will be nearly equal in magnitude (see Coursey, 1987). Additionally, the endowments might be expected to provide "anchors" and cause participants to simply give back the money they were provided to the experimenter. As will be seen in the next section, there is little evidence to support this anchoring conjecture.

[10] In the field surveys, 26 percent of the individuals surveyed using the CVM instrument indicated a zero bid because they *favored* the baseline level of 200 trees, while 11 percent bid zero because they felt that taxes should cover any incremental change of trees. For the SAF surveys, 27 percent of the individuals bid zero because they favored the original plan, while 6 percent felt that taxes should support any change in tree increments. Overall, the CVM, SAF, and SAL initial field contacts for participation, yielded a 98 percent positive response rate. No individual in the laboratory declined to participate in the Smith auction. Included in Table 2 is a report of zero bids obtained in the laboratory experiments. All of the data collected over the three elicitation techniques were used in subsequent analysis.

TABLE 2—LABORATORY EXPERIMENTS: SUMMARY STATISTICS

	Experiment	*n*	Initial Endowment	Group Fund	Summary Statistics	Trial				
						1	2	3	4	5
Willingness-to-Pay Experiments	1	12	30	111.96	Mean Bid	9.58	8.41	9.17	9.52[a]	–
					Median Bid	10.00	9.00	10.00	10.00	–
					Sum of Bids	115.00	100.90	110.00	114.25	–
					No. Zero Bids	2	2	2	1	–
25 Tree Increment	2	5	30	46.65	Mean Bid	1.87	1.87	3.00	3.40	4.00
					Median Bid	0.00	0.00	0.00	0.00	0.00
					Sum of Bids	9.33	9.33	15.00	17.00	20.00
					No. Zero Bids	4	4	4	3	3
50 Tree Increment	1	9	45	168.12	Mean Bid	6.11	11.67	10.22	11.67	12.78
					Median Bid	0.00	15.00	15.00	15.00	5.00
					Sum of Bids	55.00	105.00	92.00	105.00	115.00
					No. Zero Bids	5	4	4	4	4
	2	3	45	55.98	Mean Bid	15.00	15.00	13.33	13.33	13.33
					Median Bid	15.00	15.00	10.00	10.00	10.00
					Sum of Bids	45.00	45.00	40.00	40.00	40.00
					No. Zero Bids	1	1	1	1	1
Willingness-to-Accept Experiments	1	8	15	74.64	Mean Bid	25.45	25.33	20.12	13.87	13.14
					Median Bid	10.00	9.67	7.67	5.50	5.00
					Sum of Bids	203.61	202.61	160.97	110.97	105.14
					No. Zero Bids	0	1	0	1	1
25 Tree Increment	2	4	15	37.32	Mean Bid	35.00	20.00	23.75	25.00	26.75
					Median Bid	37.50	15.00	22.50	25.00	25.00
					Sum of Bids	140.00	80.00	95.00	100.00	107.00
					No. Zero Bids	0	1	0	0	0
50 Tree Decrement	1	5	15	93.50	Mean Bid	33.89	33.12	32.14	22.73	18.15[b]
					Median Bid	18.75	18.60	18.60	18.66	18.60
					Sum of Bids	169.45	165.60	160.70	113.66	90.76
					No. Zero Bids	0	0	0	0	0
	2	8	15	148.28	Mean Bid	88.13	145.75	179.38	148.38	143.88
					Median Bid	32.50	26.50	27.50	25.00	25.00
					Sum of Bids	705.00	1166.00	1435.00	1187.00	1151.00
					No. Zero Bids	0	0	0	0	0

[a] Sum of Bids exceeded group fund, experiment concluded in Trial 4.
[b] Sum of compensation requested was less than group fund, experiment concluded.

sion in the number of trees when values are estimated in a CVM environment. There is approximately a seventy-fivefold difference between compensation-demanded and willingness-to-pay values. This finding is consistent with the loss-aversion phenomenon described by Kahneman and Tversky. We therefore fail to accept our first hypothesis that a nonmarket-like elicitation environment will yield symmetric valuation measurements.

The addition of the market-like demand revealing properties to the hypothetical survey environment, as captured in the SAF design, also yields values consistent with loss-aversion phenomenon. Indeed, the SAF results obtained in the field are consistent with the more traditional survey responses obtained using the CVM technique. This conclusion is supported both by the summary statistics comparing CVM and SAF, and in a frequency comparison of CVM and SAF results. Apparently, using only a limited addition of the demand-revealing properties is insufficient to accept our second hypothesis that value measurements will be symmetric around the baseline level of trees.

Laboratory results are less favorable to the contention that loss aversion is prevalent in the valuation measures. Using the final trial values obtained in the experiments, the ratio between compensation demanded and willingness-to-pay measures of value drops to approximately five to one. This ratio is significantly smaller than the seventy-five-to-one ratio obtained using the field instruments. The ratio of five-to-one in valuation measurements does not reflect perfect statistical symmetry between compensation demanded and willingness to pay, but does suggest that the value of trees is more consistent with smoothly declining marginal utility.

The dynamics of the bidding observed during the repetitive laboratory auctions strongly indicate a decrease in the amount of money requested for compensation. As indicated in Table 2, mean bids for compensation begin in trial 1 at the same level as observed in the SAF surveys. However, these requests for compensation decline as the number of trials in the experiment increases. The willingness-to-pay values increase during each trial of the experiments, but not to the same magnitude that we observe the willingness-to-accept values declining.

Typical individual behavior in the laboratory experiments may be described as follows. In a willingness-to-pay experiment, some individuals consistently contribute nothing to the expansion. Most individuals in such an experiment exhibit a tendency to increase the amount of their contribution for the tree expansion program. A few other participants contribute a constant amount to the expansion. Likewise, a typical willingness-to-accept experiment indicates that a majority of individuals dramatically reduce their requests for compensation over the course of the experiment. It is important to note also that none of the obvious anchor points in the willingness-to-pay experiments were consistently used as a bid by the individuals. Possible anchors in the valuation process such as the $15 the individuals received for participating in the experiment or the $30 including the endowment which they had during the experiment, were not a focal point in bidding behavior. Also, as was typical in these experiments, some individuals bid more than their total endowment.

To evaluate the effects of economic and social variables upon bidding behavior over the three elicitation procedures, ordinary least squares estimation was conducted. The results of this estimation are reported in Table 3.[11,12] Three independent variables in these regressions are relevant for considering the three hypotheses outlined in Section I. The coefficient on tree-increment quantity reported for the two regressions provides a measure of the value functions slope in the CVM elicitation environment. Both of these measures are positive as expected, but the slope in the willingness-to-accept domain, 34.5, dominates the slope in the willingness-to-pay domain, 0.27. The coefficient on tree-increment quantity multiplied by the dummy variable *SAF*, with *SAF* being one if the bid was obtained using the *SAF* elicitation technique and zero otherwise, indicates the marginal change in slope of the valuation function using the *SAF* survey instrument. Changes in slope in the willingness-to-pay and willingness-to-accept domains are both

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[11] Our criterion for including variables in both of these regressions was that the t-value of the coefficient on an independent variable had to be at least 1.00 in absolute value in one of the WTP and WTA regressions. Variables not reported in Table 3 which did not satisfy this requirement were education of the primary household wage earner and distance from the park (either in terms of the respondent's estimate of how long it would take to travel to the park, straight-line distance measured on a map, or minimum street distance to the park measured on a map). We also tested whether interview team bias had any effect upon the data collected. Using a dummy variable to control for the interview teams we found no significant interviewer effects. Finally, the willingness-to-pay regression was also estimated with the subset of the data which eliminated bids which indicated that taxes should pay for the proposed plan. This elimination had no significant statistical effects upon the estimates.

[12] WTP bids were positive and were regressed against positive tree increments. WTA bids were expressed as negative quantities and were regressed against negative tree increments. This allows us to compare the coefficients of the tree-increment variables directly. It is important to remember that bids are expressed in negative amounts for the willingness-to-accept regression when comparing other variables. For example, the positive coefficient on income in the willingness-to-pay experiments indicates that trees are a normal good. The negative coefficient on income in the willingness-to-accept regression indicates that as income increases, more compensation will be required to remove trees. This also is consistent with an interpretation of trees as a normal good.

TABLE 3—MODELS FOR WILLINGNESS-TO-PAY AND WILLINGNESS-TO-ACCEPT BIDS

	Dependent Variable			
	WTP Bids		WTA Bids	
Independent Variable	Coefficient	Elasticity	Coefficient	Elasticity
View	−0.59	−0.0645	−333	0.512
0 = Total View, 1 = Partial View, 2 = No View	(−0.35)		(−1.15)	
Household Population	1.20	0.263	263	−0.811
	(1.17)		(1.83)	
Race	4.99	0.0365	132	−0.00994
0 = White, 1 = Nonwhite	(2.47)		(0.29)	
Monthly Household Income after Taxes	0.0029 (2.76)	0.407	−0.26 (−1.55)	0.549
Tree-Increment Quantity	0.27	0.695	34.5	1.27
	(3.20)		(2.31)	
Tree-Increment Quantity × SAF	−0.096	−0.0951	−1.26	−0.0186
	(−1.52)		(−0.12)	
Tree-Increment Quantity × SAL	−0.16	−0.0922	−31.3	−0.248
	(−2.14)		(−2.40)	
Any Members of Household Attending School near Troutman Park	−4.65 (−1.86)	−0.157	480 (1.19)	−0.225
Sample Size	248		234	
F-statistic	29.6		5.95	

Note: The *t*-statistics are reported in parentheses, elasticities computed at means of all variables.

insignificant. The coefficient on tree-increment quantity multiplied by the dummy variable *SAL*, with *SAL* being one if the bid was obtained using the *SAL* elicitation technique and zero otherwise, indicates the marginal change in slope of the valuation function using the repetitive laboratory procedure. In both domains, the shift in slope is significant. Most important, the willingness-to-accept slope, when measured in the *SAL* elicitation environment, falls significantly. Thus, the overwhelming support for value asymmetry obtained in testing our first two hypotheses is not substantiated in a test of our Hypothesis 3.[13,14]

IV. Conclusions

Our consideration of the frame of reference effects associated with measuring the value of a public good has focused upon a comparison of hypothetical nonmarket and actual market-like elicitation techniques. Our econometric comparisons indicate that values obtained from responses to survey questionnaires or from single iteration Smith

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[13] When examining Table 1 and relative frequency plots of WTP and WTA bids, it appeared that outliers and the potential for skewed residual distributions might have been important in affecting the results reported in Table 3. Edward Leamer (1984) has proposed a method of testing for the influence of outlying observations which leads to a *t*-test to gauge whether the estimates derived from ordinary least squares would be affected by downweighting outlying observations. When this method was applied to our data, no evidence was found to support the conjecture that outliers were the primary driving force behind the results reported in Table 3. The actual regression results are available upon request of the authors.

[14] Contrary to our expectations, the slope of willingness-to-pay decreases, rather than increases, in the context of the SAL market-like setting. This is consistent with the results obtained when a public good was allocated using a Smith auction process in our paper with William Schulze (1987). At the present time we have no explanation for this phenomenon. Speculation concerning the nature of this result has centered upon the role of altruism in affecting the willingness-to-pay bids. The conjecture most commonly pronounced is that the market-like mechanism may induce a smaller degree of altruism than hypothetical measures of value.

auction processes do result in loss-aversion inconsistencies. These inconsistencies appear to be especially pronounced in the willingness-to-accept domain of public good preferences.

However, the most important conclusion to be drawn from our research is that the magnitude of the loss-aversion phenomenon is sensitive to the degree in which values are measured in a market or nonmarket environment. When the market-like elicitation process is repeated even a small number of times, values for the public good are more consistent with traditional economic notions of diminishing marginal utility. Although individuals may initially exaggerate their preferences for the public good, they modify their stated values as a function of the incentives, feedback, interactions, and other experiences associated with the repetitive auction environment. We cannot reject the hypothesis that these individuals exhibit loss-aversion behavior. However, the marketplace appears to be a strong disciplinarian in terms of limiting this type of behavior.

Prospect theory models of value provide guidance for measuring the way in which individuals' preferences become incoherent in hypothetical settings. Depending upon the form of the question asked, hypothetical survey data may provide accurate or inaccurate measures of value. Our study provides evidence which suggests that hypothetical willingness-to-pay values may be both more accurate and more stable than hypothetical willingness-to-accept values. We interpret this evidence as providing a constructive response to those who would reject all contingent valuation methods, data and implications for policy analysis in an out-of-hand fashion.

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Sex Discrimination in Labor Markets: The Role of Statistical Evidence

By PETER KUHN*

Correlations between statistically measured wage discrimination and confidential survey reports of discrimination are used to estimate the relative importance of statistical evidence in determining women's self-assessed discrimination levels. Statistical evidence is found to be less important than other, "nonstatistical" evidence. The estimates indicate that the percentage of women who report discrimination would fall from 15.4 to 10.2 percent in Canada, and from 9.9 to 6.3 percent in the United States if all conventionally measured pay discrimination were absent.

In recent years, several statistical measures of sex discrimination in labor markets have been developed. Among economists, the most widely used technique,[1] developed by Ronald Oaxaca (1973), uses regression analysis to estimate the proportion of the male-female wage differential that is not attributable to observable differences in the human capital, or "qualifications," of workers.[2]

Since the Equal Pay Act of 1963 and the Civil Rights Act of 1964, U.S. courts have also been faced with the problem of assessing sex discrimination experienced by individuals, or by groups involved in class-action suits. Throughout this period, the admissibility of various kinds of statistical evidence in the courts has been a subject of considerable controversy. For example, statistical evidence of underrepresentation in promotion or hiring, which has long played an important role in affirmative action programs under the Office of Federal Contract Compliance Programs (OFCCP),[3] has recently been challenged by members of the Reagan Administration who claim it "proves little or nothing about employment discrimination."[4] Still, it was ultimately ruled admissible by the Supreme Court in two recent decisions (*International Association of Firefighters v. Cleveland*, 1986; *Sheet Metal Workers v. Equal Employment Opportunity Commission*, 1986).[5]

Statistical evidence of wage discrimination, not admissible under the Equal Pay Act, may become admissible in coming years as a result of at least two recent Supreme Court decisions. The first of these (*County of Washington v. Gunther*, 1981), ruled that claims of sex discrimination in wages can be

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[1] Alan Blinder (1973), Jacob Mincer and Solomon Polachek (1974), and Mary Corcoran and Greg Duncan (1979) are among the more widely cited applications.

[2] Other techniques include "utilization analysis," which estimates discrimination in hiring and promotion by comparing minority representation among accepted workers to that in a qualified applicant pool, and "comparable worth" studies which compare wages of men and women in jobs rated equally "valuable" according to a job evaluation scheme. See Jonathan Leonard (1985) for a discussion of representation analysis, and Donald Treiman and Heidi Hartmann (1981, ch. 4) for a discussion of comparable worth analysis.

[3] Leonard shows that, despite official guidelines, the OFCCP does not appear to use representation of minorities in selecting *which* firms to review. Still, representation analysis forms an important part of the review procedure itself and is important in establishing guidelines for the reviewed firm to follow.

[4] See R. Pear (1985).

[5] See S. Taylor (1986) for an outline of these decisions and their ramifications.

brought under Title VII of the Civil Rights Act even though no male in the same firm holds an "equal" but higher-paying job. This opened the door for statistical techniques that compare women's wages to those of men with equal qualifications in different jobs, as well as to males in other firms. More recently, in *Bazemore et al. v. William Friday et al.* (1986), the Court ruled specifically on regression analyses of pay differentials, arguing that such analyses could not be dismissed simply because they do not include *all* relevant variables. Thus it seems likely that wage regressions will play an increasing role in future U.S. discrimination cases.[6]

If statistical evidence of wage discrimination is allowed in court, how much weight should be accorded to it, relative to the various kinds of nonstatistical evidence currently being used? And what would be the likely effects of allowing statistical evidence on women's litigation and success rates in wage-discrimination suits? This paper argues that some insights into these questions can be gained from an analysis of individual reports of discrimination (i.e., women's answers to the question "Do you feel discriminated against on your job because of your sex?" in a confidential survey). More specifically, it estimates the relative importance of statistical[7] vs. other (i.e., "nonstatistical") evidence in determining women's *self-assessed* discrimination levels. To the extent that survey reports are honest estimates of "true" discrimination, this information could be useful in judging how these two types of evidence should be treated by the courts. In addition, if we expect the courts, given admissibility, to weigh statistical vs. nonstatistical evidence in the same way that individuals do, it may be useful in predicting the likely effect of admissibility on women's success and litigation rates in wage-discrimination suits.[8]

The paper's main results are as follows. First, it is clear that, in both data sets considered here, *nonstatistical* evidence of discrimination is a more important determinant of women's self-assessed levels of discrimination than the level of statistically measured wage discrimination. This finding is reflected in my estimate that, even if conventionally measured wage discrimination were completely eliminated from labor markets, the percentage of women with median characteristics who report discrimination would fall only from 15.4 to 10.2 percent in Canada, or from 9.9 to 6.3 percent in the United States.

Second, I find that the level of nonstatistical evidence of discrimination experienced by women varies considerably with their observed characteristics in the following way: young, well-educated women appear to experience much more nonstatistical evidence of discrimination, and as a result are much more likely to report discrimination than others. This provides an interesting perspective on findings such as those in Jonathan Leonard (1985), which indicate that U.S. antidiscrimination policy is targetted disproportionally at women in highly paid occupations. Specifically, it suggests that this targetting policy may reflect a sensitivity by policymakers to evidence of discrimination that is not captured by standard wage regression techniques.

The paper proceeds as follows. In Section I, I generate "naive" estimates of the importance of statistical evidence to individuals by simply running a probit of reported discrimination on conventional statistical measures. This yields a rather puzzling result: in all cases, women for whom there is greater statistical evidence of discrimination are *less* likely to report feeling discriminated against. Section II examines the sensitivity of this result to certain simple specification changes that arise naturally out of certain objections one might raise to the basic measure of wage discrimination. It finds the result to be robust. Section III resolves this puzzle by developing and estimating a formal structural model of the decision by a woman to report discriminatory treatment which both (a) gives a coherent theoretical interpretation to the cross-sectional pattern of reports vs. statistical measures, and (b) includes the naive model as a (not particularly compelling) special case.

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[6] For a more detailed discussion of these legal issues, see Mary Heen (1984), and Orley Ashenfelter and Oaxaca (1986).

[7] Throughout the paper, I use wage regressions as my statistical measure of discrimination. The same analysis can in principle be performed using other statistical measures, such as underrepresentation measures or comparable worth studies. The focus on wage regressions here is because (a) in the *QES*, "wage and salary inequities" is the most frequent reason for reporting discrimination (see my 1985 paper), and (b) this estimate is probably of greatest interest to economists.

[8] Strictly speaking, the implications of the present paper's results for litigation and success rates can be rigorously derived only by postulating a formal, utility-maximizing model of the litigation decision and the Court process. Such an analysis is presented in a previous version of this paper (1987). The present paper simply suggests what appear to be the most obvious implications; these are shown to be compatible with a formal model in the earlier piece.

I. Basic Results

A. *Measuring Discrimination on an Individual Basis*

"Standard" estimates of wage discrimination are based on the following two regressions:

$$(1) \qquad w_i^F = \sum_j X_{ij}^F \beta_j^F + \mu_i^F$$

$$(2) \qquad w_i^M = \sum_j X_{ij}^M \beta_j^M + \mu_i^M$$

where the superscripts stand for female and male, respectively, and w_i^F and w_i^M are the logs of individual i's wage rate. The X_{ij}s include the standard human capital variables, and throughout the paper I adopt the convention that X_{i0} is a vector of ones. When (1) and (2) are estimated by ordinary least squares yielding the coefficients $\hat\beta_j^F$ and $\hat\beta_j^M$, the estimate of discrimination experienced by the "average" female developed by Oaxaca, and used by many researchers since then may be expressed as

$$(3) \qquad \hat{\bar{D}} = \sum_j \bar{X}_j^F \hat\beta_j^M - \sum_j \bar{X}_j^F \hat\beta_j^F = \sum_j \bar{X}_j^F \hat\beta_j^M - \bar{w}^F$$

where $\bar{X}_j^F$ and $\bar{w}^F$ denote the sample means of characteristic X_j and of the log wage, respectively. $\hat{\bar{D}}$ of course incorporates the fact that returns to observed characteristics may differ for men and women, and indeed estimates the extent to which this occurs.

For the purposes of this paper, individual-specific measures of discrimination are required. Two such measures, based on (3), are $\hat{D}_i^1$ and $\hat{D}_i^2$, defined by

$$(4) \qquad \hat{D}_i^1 = \sum_j X_{ij}^F \hat\beta_j^M - w_i^F = \sum_j X_{ij}^F \hat\beta_j^M - \left(\sum_j X_{ij}^F \hat\beta_j^F + \hat\mu_i^F\right) = \hat{D}_i^2 - \hat\mu_i^F,$$

where

$$(5) \qquad \hat{D}_i^2 = \sum_j X_{ij}^F \hat\beta_j^M - \sum_j X_{ij}^F \hat\beta_j^F$$

and $\hat\mu_i^F$ is the estimated residual in the female earnings equation (1).

Both $\hat{D}_i^1$ and $\hat{D}_i^2$ are estimates of the difference between what a specific woman actually earns, and what she would earn "if she were a man" with the same characteristics as herself. The essential difference between them is in what they assume about the returns to a women's *unobserved* characteristics, or "unmeasured ability," μ_i^F, in the male vs. female sectors, or "regimes." If unmeasured ability is sector-specific, then a woman who earns $w_i^F = \sum_j x_{ij}^F \beta_j^F + \mu_i^F$ would earn simply $\sum_j X_{ij}^F \beta_j^M$ if she were a man, and $\hat{D}_i^1$ is the appropriate estimate of her relative earnings in the two sectors. If unmeasured ability is "general," then a woman who earns $w_i^F = \sum_j X_{ij}^F \beta_j^F + \mu_i^F$ will earn $\sum_j X_{ij}^F \beta_j^M + \mu_i^F$ as a man, and $\hat{D}_i^2$ is appropriate. Since, *ex ante*, there is no compelling reason to prefer $\hat{D}_i^1$ over $\hat{D}_i^2$, or vice versa, I perform most of the analysis below using both measures.

B. *Data*

The 1977 American *Quality of Employment Survey* (*QES*) (Robert Quinn and G. Staines, 1979) is a nationwide-probability sample of 1515 individuals who normally worked for pay 20 or more hours per week (i.e., "full-time" workers), and were 16 years of age or older. When all nonwhites, self-employed, and nonusable responses[9] are excluded from this data set, we are left with a sample of 619 men and 373 women, used in the entire analysis. A woman in this data set is defined as having reported sex discrimination in the labor market ($RD_i = 1$; otherwise $RD_i = 0$) if she answers "yes" to the question: "Do you feel in any way discriminated against on your job because you are a woman?"

The 1977 Canadian *Quality of Life Survey* (Tom Atkinson et al., 1984) is a nationwide probability sample[10] of 3288 persons aged 18 years and over, that reports data on wages and employment for all individuals who normally worked 20 or more hours per week, whom I shall henceforth call "participants," or full-time workers. When all nonwhites, self-employed, unpaid workers, individuals over 76 years of age, and unusable responses are excluded from this data set, we are left with a sample of 887 men and 858 women, of whom 658 and 448, respectively, were participants (i.e., full-time workers). The sample of full-time workers in Canada is thus selected on essentially the same criteria as the *QES* sample.

A woman is defined as having reported discrimination in the Canadian data if she answers "yes" and mentions "sex" as a reason in: "Discrimination can take place for other reasons, for example, on the basis of sex or age. Do you feel that you have ever been discriminated against in some such way?... Could you describe this discrimination?"

C. *The Relationship Between Reports and Measures*

The OLS estimates of the basic earnings regressions (1) and (2) are reported for both data sets in Table 1. Since much of the data on the independent variables are available only in categorical form, dummy variables are used extensively; throughout the analysis I have attempted to define variables as identically as possible in the two countries.[11]

The estimated earnings functions in Table 1 exhibit patterns and magnitudes of coefficients that are, by now, quite familiar to most labor economists. Several features, are, however, noteworthy. First, the mean hourly wages of the Canadian men and women are \$6.81 and \$5.19, respectively, while American men and women earned \$6.30 and \$3.74, respectively. Thus, in the Canadian sample, women earn 76.2 percent of what men earn; in the United States, that figure is 59.4 percent.[12] Second, there is no strong evidence for the lower return to human capital for women noted by Jacob Mincer and Solomon Polachek (1974) and analyzed by Shelly Lundberg and Richard Startz (1983) in my data. Returns to education appear comparable for men and women in both countries; returns to on-the-job experience are higher for women than men in Canada, while the reverse is true in the United States. Third, there is a considerable difference in the mag-

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[9] Exact age and earnings are unavailable for individuals over 77 years old, or those earning over \$95,000 annually. These are treated as unusable responses, as are all observations with missing data on any of the variables used. As well, the U.S. data contained a number of individuals whose wages were obviously misreported. These individuals were eliminated by retaining only individuals whose log hourly wage rates were between 0 and 3.9. This latter exclusion does not greatly affect the results (most of the analysis was done with and without it) but greatly facilitated the computation of the maximum likelihood models in Section III. A similar restriction was not required in the *QES*, which seems to have much more accurate wage data. See the Appendix for a more detailed description of how the data sets were constructed.

[10] The data used here include an extra "urban sample" of Toronto and Montreal as well.

[11] An earlier version of this paper (1985) also estimated earnings functions using as much continuous data as possible for each country, with little change in the results.

[12] Given the similarity of the selection criteria used for both the Canadian and U.S. models, the relatively low average level of discrimination in Canada is surprising and somewhat hard to explain. One possibility is the oversampling of large urban areas in the Canadian data set, which might also explain the discrepancy between these results and 1981 Canadian census results, which has been pointed out by a referee.

TABLE [illegible]—OLS EARNINGS FUNCTIONS ESTIMATES

		Canada				United States			
		Men		Women		Men		Women	
		Mean	Coefficient	Mean	Coefficient	Mean	Coefficient	Mean	Coefficient
	Intercept		1.260 (.110)		1.165 (.120)		.871 (.113)		.630 (.127)
Age:[a]	25–34	.318	.095 (.053)	.328	.116 (.051)	.325	.080 (.064)	.284	.045 (.063)
	35–44	.210	.227 (.062)	.194	.156 (.061)	.202	.115 (.101)	.201	.046 (.076)
	45–54	.161	.195 (.066)	.170	.024 (.064)	.179	.063 (.126)	.158	−.143 (.089)
	55–64	.143	.180 (.070)	.071	.024 (.086)	.120	−.073 (.153)	.105	−.172 (.096)
	65–76	.012	−.607 (.157)	.067	−.190 (.224)	.011	−.067 (.233)	.013	−.367 (.185)
	Age in Years	38.1		35.0		39.6		41.1	
Education:[b]	8 years	.097	.017 (.088)	.069	−.156 (.111)	.034	.051 (.121)	.035	.149 (.147)
	9–11 years	.239	.216 (.078)	.188	.064 (.096)	.129	.206 (.098)	.121	.216 (.121)
	12 years	.295	.274 (.080)	.397	.175 (.093)	.367	.306 (.092)	.413	.372 (.113)
	13–15 years	.146	.420 (.084)	.156	.267 (.099)	.228	.443 (.095)	.206	.441 (.117)
	16 years	.097	.613 (.089)	.105	.539 (.103)	.102	.588 (.101)	.083	.519 (.129)
	17+ years	.074	.655 (.093)	.038	.581 (.126)	.108	.545 (.100)	.110	.777 (.126)
Tenure with Current Employer[c]	1 year [d]	.116	.050 (.085)	.172	.138 (.079)	.126	.059 (.065)	.177	.034 (.067)
	2–3 years	.193	−.008 (.079)	.252	.132 (.075)	.192	.138 (.061)	.188	.087 (.067)
	4–5 years	.143	.175 (.082)	.112	.144 (.084)	.118	.078 (.068)	.126	.287 (.077)
	6–10 years	.173	.153 (.082)	.199	.253 (.080)	.173	.273 (.064)	.177	.205 (.074)
	11–20 years	.179	.241 (.084)	.147	.424 (.086)	.192	.240 (.066)	.142	.313 (.085)
	21+ years	.143	.287 (.091)	.042	.401 (.116)	.099	.342 (.079)	.048	.232 (.120)
	Tenure in years	9.44		6.02		–		–	
Experience in Total Labor Market	Experience in years[e]					19.9	.029 (.009)	13.1	.009 (.009)
	Experience squared					557.9	−.00044 (.00018)	266.0	.00010 (.00021)
	Previous experience dummy[f]	.723	.059 (.038)	.600	−.061 (.039)				
Trade School[g]		.096	.071 (.063)	.103	.017 (.064)	.262	−.017 (.037)	.161	−.082 (.053)
Union Membership[h]		.471	.089 (.033)	.339	.112 (.040)	.331	.172 (.034)	.223	.186 (.049)
Physical Disability[i]		.219	−.022 (.040)	.208	.060 (.045)	.103	−.015 (.052)	.126	−.096 (.059)
Log (*Wage*)	(dependent variable)[j]	1.918		1.647		1.840		1.319	
Number of Observations		658	658	448	448	619	619	373	373
$\bar{R}^2$			.246		.309		.319		.374

Note: Standard errors are shown in parentheses.

[a] Omitted group is aged 18–24 in Canada. 16–24 in the United States.

[b] Omitted group has no schooling or incomplete grade school.

[c] Omitted group is less than one year in Canada, less than 3 months in the United States.

[d] Three months to one year in the United States.

[e] Total years worked for pay since age 16.

[f] Equals one if reported a first full-time job in a different industry or occupation from current job; equals zero otherwise.

[g] "Technical School" Dummy. In Canada, equals one if reported "technical training beyond secondary school." In United States, equals one if reported "ever having gone to a trade school."

[h] In Canada equals one if belongs to a labor union, zero otherwise. In United States, equals one if belongs to a union, or "employee's association similar to a union."

[i] In Canada, equals one if reports "long-standing health trouble or physical disability." In United States, equals one if reports any "physical or nervous condition that limits the amount or kind of work you do."

[j] Natural logarithm of hourly wage on main job.

TABLE 2—PROBIT ESTIMATES OF EFFECTS OF MEASURED DISCRIMINATION ON REPORTED DISCRIMINATION

	Canada			United States		
	Means	Coefficients		Means	Coefficients	
Intercept		$-.977$ (.079)	$-.994$ (.133)		$-.965$ (.113)	$-.677$ (.219)
$\hat{D}_i^1$	.207	$-.226$ (.182)		.396	$-.428$ (.228)	
$\hat{D}_i^2$	.207		$-.122$ (.542)	.396		-1.1628 (.544)
RD_i (dependent variable	.154			.131		
Number of observations	448	448	448	373	373	373
Log likelihood		-191.72	-192.44		-143.30	-142.75

nitude of the union status coefficients, which are considerably higher in the United States.

A convenient way to summarize the cross-sectional relationship between reported and measured discrimination in the two samples is to estimate a probit equation of the form:

$$(6)\quad \text{Prob}\left(RD_i=1|\hat{D}_i\right) = \text{Prob}\left(\epsilon_i \leq m + n\hat{D}_i\right),$$

where $\epsilon_i \sim N(0,1)$ is an individual-specific random variable assumed to be independent of $\hat{D}_i$, and m and n are unknown parameters. The extent to which the slope parameter n exceeds (is less than) zero then indicates the extent to which women with higher measured discrimination are more (less) likely to report being discriminated against.

Table 2 reports the results of the probit analyses in (6), when performed on measures of discrimination calculated from the regressions in Table 1. The most striking feature of the table is its very clear rejection of the notion that traditional "calculated" measures of discrimination are positively related to reported discrimination—in fact, all estimates of the relationship are negative, although only one of them has a t-ratio greater than two in absolute value. Since the standard errors in Table 2 underestimate the true standard errors (see Adrian Pagan, 1985), it is likely that none of these coefficients differs significantly from zero.

To judge the magnitude of the estimated effects in Table 2, consider the largest in absolute value, -1.1628. For a women experiencing the average level of $\hat{D}_i^2$ in the United States (.396), this implies that the marginal effect of $\hat{D}_i^2$ on the probability of reporting is -2.422. In other words, a 10 percent decrease in the wage this woman receives, holding her qualifications (i.e., her wage "as a man") fixed, is estimated to lower her probability of reporting by 2.4 percentage points, from 12.7 to 10.3 percent. Thus none of the effects in Table 2 seem particularly large in magnitude.

II. Alternative Measures of Discrimination

Two possible objections to the technique of the previous section are as follows: 1) The statistical measures of discrimination used above are biased and inconsistent because the earnings functions on which they are based, especially for women, could be subject to selectivity bias; 2) The statistical measures of discrimination are biased and inconsistent because the earnings functions control for the wrong set of characteristics, some of which may be viewed as endogenous outcomes of discrimination.[13] This section

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[13] A third possible source of bias not analyzed here is classical measurement error in the X's. While some analysts have proposed reverse regression techniques to supply an upper bound to the bias on overall discrimination (see, for example, R. A. Kamalich and

TABLE 3—PROBIT ESTIMATES OF THE EFFECT OF MEASURED DISCRIMINATION ON REPORTED DISCRIMINATION USING ALTERNATIVE MEASURES OF DISCRIMINATION

	Probit Coefficient of RD_i on $\hat{D}_i$			
	Canada		United States	
	$\hat{D}_i^1$	$\hat{D}_i^2$	$\hat{D}_i^1$	$\hat{D}_i^2$
1) Discrimination Measures Based on Selectivity-Corrected Earnings Regressions[a]	−.225	−.104	n.a.	n.a.
	(.182)	(.529)		
2) Discrimination Measures that Control for Only a Subset of Observed Characteristics				
(a) Tenure not held constant	−.214	−.086	−.485	−1.493
	(.177)	(.439)	(.230)	(.555)
(b) Tenure and experience not held constant	−.243	−.266	−.550	−1.321
	(.178)	(.465)	(.215)	(.424)
(c) Tenure, experience, education and trade school not held constant	−.468	−.996	−.582	−1.142
	(.159)	(.295)	(.200)	(.341)

Note: Standard errors are shown in parentheses.

[a] Aside from all the variables in Table 1, earnings functions also include Heckman's λ term. This was calculated from a reduced-form probit of labor force participation on all the variables in Table 1 plus the following: marital status dummies; number of children living at home aged 0–5, 6–13, 14+, respectively; and exogenous family income, measured as total family income minus personal income of the respondent.

considers the sensitivity of the "basic" result of these two specification changes in turn.

A. *Selectivity Bias*

It is well known that, given data on individuals not included in the sample, sample selection bias can be corrected using James Heckman's (1979) two-stage technique. Since data on nonparticipants are not available in the *QES*, this correction is done here only in the Canadian data. The resulting earnings equations are almost identical to the OLS-based results in Table 1; indeed, their main feature is a consistently insignificant coefficient on the sample selection variable, λ, in both the male and female earnings regressions. This explains why, when the probit (6) is reestimated using measures of $\hat{D}_i$ based on selectivity-corrected earnings regressions, as in row 1 of Table 3, very little change in the results occurs. Thus selectivity bias in the earnings function does not appear to be responsible for the negative association found in the basic model here.

B. *Controlling for Other Sets of Characteristics*

Thus far in the analysis I have assumed that, in assessing whether she is discriminated against, a woman compares her earnings to those of a man who is identical to her in terms of *all* her measured characteristics, X_i^F. What if, instead, women perceive some of the male-female differences in the X's, such as tenure, to be themselves the result of discrimination? To check whether this is responsible for the basic model's results I reestimated the male earnings function (1), *excluding* various sets of variables women might perceive as resulting from discrimination, which allows one to compute a woman's expected earnings as a man, *not* conditional on having the same levels of these excluded

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Polachek, 1982) A. Goldberger (1984) has argued that their technique is incorrect. It thus seems unwise to attempt to correct for this problem in the present context before a better technique is developed.

characteristics.[14] The results are shown in rows (a)–(c) of Table 3. Since this adjustment only makes the effect of measures on reports *more* negative and significant, the use of an excessive number of controls does not appear to provide a simple "explanation" of my basic result either.

III. A Structural Model of Wages, Discrimination, and Reporting Behavior

A. *The Model*

Since the surveys under consideration are both confidential and took place well before the court decisions referred to above, I posit a formal model of reporting behavior which ignores strategic reasons[15] for reporting discrimination and assumes women attempt to give the survey researcher an honest assessment of the "total" amount of discrimination they face.[16] Total discrimination, Δ_i, is defined as the difference between the total labor market "success" of a man and woman with comparable characteristics, measured in (log) wage-equivalent units.

In assessing the level of total discrimination she faces, I assume a woman has access to two types of information. One of these is D_i, the difference between her own wage and that of a man with comparable levels of X_{ij} (i.e., a standard measure of wage discrimination). The other, ω_i, includes differences in nonwage benefits, the type of treatment received on the job, as well as other information about wage discrimination not embodied in D_i. These "other" types of information, although not directly observable by survey researchers, are nonetheless potentially verifiable in a court case, and are likely to constitute important evidence in such a situation. Indeed, if D_i is inadmissible in court, they constitute the only source of evidence. For ease of reference (and in line with current popular usage), I shall henceforth refer to D_i as statistical evidence of discrimination, and ω_i as nonstatistical evidence.

A woman's best estimate of the total discrimination she faces is assumed to be given by

$$\hat{\Delta}_i = \omega_i + \alpha D_i, \tag{7}$$

where α (positive or negative) is the weight a woman gives to the level of the standard statistical measure of discrimination in making her estimate of true, total discrimination.

The true level of discrimination (again positive or negative) experienced by individual i is observed by no one, but is known to be given by

$$\Delta_i = \hat{\Delta}_i + e_i, \tag{8}$$

where $e_i \sim N(0, \sigma_e^2)$ and is independently and identically distributed. I assume a woman reports discrimination in a survey if and only if she is at least k_i percent certain, based on all the information to which she has access, that her true Δ_i exceeds zero. In other words,

$$RD_i = 1 \text{ iff } \hat{\Delta}_i > z(k_i), \tag{9}$$

where z is the critical value of the normal distribution with variance σ_e^2 and mean zero for a one-tailed test of the null hypothesis that $\Delta_i = 0$ with level of significance k_i percent.

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[14] Alternatively, one could argue that the earnings regressions should include *more*, rather than fewer, characteristics, such as a woman's firm, region, or occupation, since in assessing whether they are discriminated against, women may compare themselves only to men in their own firm, or some appropriately defined reference group. Unfortunately, it is not feasible to include these controls given the two data sets here.

[15] For example, if the reports under consideration were public information, some women might refrain from reporting out of fear of dismissal. Others (particularly those with a comparative advantage at litigation) might be more willing to report because reporting may ultimately raise wages. If, perhaps because respondents distrust the confidentiality of the survey, some "strategic" motivations of this kind are present in our data, this alters the interpretation of the results. See fn. 19.

[16] Given a confidential survey, individuals of course have little or no incentive either to lie or tell the truth. I assume, as do *all* researchers relying on self-reported data on wages, incomes, hours, etc., that respondents report the truth.

Finally, I assume that

$$\omega_i = \sum_j \theta_j X^F_{ij} + \phi^1_i; \tag{10}$$

$$z(k_i) = (\bar{z} + \phi^2_i)\sigma_e, \tag{11}$$

where ϕ^1_i and ϕ^2_i are normally distributed error terms with mean zero. Both are independent of X^F_{ij} and of each other; but they may be correlated with u^F_i. In (10) and (11), the level of "nonstatistical" evidence of discrimination may vary systematically with a woman's observed characteristics, while, for simplicity in interpreting the results, the "level of significance" women choose for their reporting decision may not.[17]

Substituting (10) and (11) into (7) and the result into (9) yields the reporting decision:

$$RD_i = 1 \text{ iff: } \alpha D_i + \sum_j \theta_j X_{ij} + \phi^1_i > (\bar{z} + \phi^2_i)\sigma_e \tag{12}$$

or

$$v_i < \alpha D_i + \sum_j c_j X_{ij}.$$

In (12), each c_j coefficient for $j \neq 0$ equals θ_j, the effect of characteristic X_j on the level of nonstatistical evidence. The constant term $c_0 = \theta_0 - \bar{z}\sigma_e$, reflecting both the overall level of nonstatistical evidence and the average degree of confidence implicit in women's reporting decisions. The error term $v_i = \sigma_e \phi^2_i - \phi^1_i$ reflects individual shocks to nonwage evidence and to confidence levels. It is normally distributed with mean 0 and variance σ^2_v.

To derive the likelihood function for the model, it is helpful to rewrite the male and female earnings equations (1) and (2) as the single equation:

$$w_i = \sum_j a_j X_{ij} + \sum_j b_j (X_{ij} y_i) + y_i \mu^M_i + (1 - y_i)\mu^F_i, \tag{13}$$

where $y_i = 1$ if individual i is male, and zero otherwise. In this formulation, the a_js are the female earnings function coefficients, β^F_j, and the b_js give the difference between the male and female coefficients, $\beta^M_j - \beta^F_j$. This allows us to write the computed statistical measures of discrimination as

$$D^1_i = \sum_j b_j X_{ij} - \mu^F_i; \tag{14}$$

$$D^2_i = \sum_j b_j X_{ij}, \tag{15}$$

depending on whether unobserved ability, u^F_i, is assumed to be sector-specific or general.

For the case $D_i = D^2_i$, which I assume without loss of generality henceforth,[18] woman i's reporting decision can now be written

$$RD_i = 1 \text{ iff } v_i < \sum_j (\alpha b_j + c_j) X_{ij}. \tag{1}$$

Thus, reporting is made more likely by positive shocks to nonwage evidence, ϕ^1_i and negative shocks to the confidence level, ϕ^2_i, (both producing lower v_i); by high levels of wage and nonwage evidence ($\Sigma_j b_j X_{ij}$ and $\Sigma_j \theta_j X_{ij}$) as well as by a low overall level of confidence in the reporting decision (i.e., a low $\bar{z}$, which raises $c_0 = \theta_0 - \bar{z}\sigma_e$). The log likelihood of the ith observation in the en-

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[17] Since the effect of, say, X_j on nonstatistical evidence is not identified separately from its effects on the significance level, $z(k_i)$, this latter restriction only affects the interpretation of the empirical results. I prefer to attribute systematic variation in reports that is not explained by $\hat{D}_i$ to systematic variations in unobserved evidence of discrimination, since I can think of no obvious reasons why significance levels should vary systematically with X's, particularly in the absence of strategic reasons for reporting discrimination.

[18] For the case $D_i = D^1_i$, the coefficient $\rho\sigma_v/\sigma_F$ in the last two terms of Table 4 is replaced by $\alpha + (\rho\sigma_v/\sigma_F)$. Since the parameter ρ is identified only by this term, the D^1_i and D^2_i models cannot be distinguished empirically. I choose the D^2_i model as a basis for interpreting the results for the following reason. The results in Tables 1 and 5 indicate few significant differences in the returns to observed characteristics for men and women. It thus seems natural to assume the same is true for unobserved ability, u^F_i.

TABLE 4—CONTRIBUTION OF ith OBSERVATION TO LOG LIKELIHOOD IN STRUCTURAL MODEL

1. Earnings function component (all observations)

$$\log\left\{\frac{1}{\sigma_M^{y_i}\sigma_F(1-y_i)} f\left[\frac{w_i-\sum_j a_j X_{ij}-\sum_j b_j X_{ij} y_i}{\sigma_M^{y_i}\sigma_F(1-y_i)}\right]\right\}$$

2. *Probit component (women only)*
   (a) Contribution of reporters ($RD_i=1$)

$$(1-y_i)RD_i\log\left\{F\left[\frac{\sum_j(\alpha b_j+c_j)X_{ij}-\frac{\rho\sigma_v}{\sigma_F}\left(w_i-\sum_j a_j X_{ij}\right)}{\sigma_v\sqrt{1-\rho^2}}\right]\right\}$$

   (b) Contribution of nonreporters ($RD_i=0$)

$$(1-y_i)(1-RD_i)\log\left\{1-F\left[\frac{\sum_j(\alpha b_j+c_j)X_{ij}-\frac{\rho\sigma_v}{\sigma_F}\left(w_i-\sum_j a_j X_{ij}\right)}{\sigma_v\sqrt{1-\rho^2}}\right]\right\}$$

Notes: The standard normal density and distribution functions are represented by f and F, respectively; and the correlation coefficient between μ_i^F and v_i is represented by ρ.

tire sample of men and women is the sum of three terms, given in Table 4.

The model in Table 4 is formally very similar to that used in Ashenfelter (1983); its parameters can be estimated either by a consistent two-stage method or by full-information maximum likelihood (FIML). The two-stage technique involves first estimating separate earnings regressions for men and women to obtain estimates of a_j, b_j, σ_M, σ_F, and $\mu_i^F=w_i-\Sigma_j a_j X_{ij}$. This corresponds to the first term in the likelihood function. The second stage consists of a probit of RD_i on X_{ij} and $\hat{\mu}_i^F$ for women only and corresponds to the last two terms in the likelihood function. Probit coefficients on X_j are consistent estimates of $(\alpha b_j+c_j)/\sigma_v\sqrt{1-\rho^2}$. These probit coefficients reflect both the effect of a variable X_j on the statistical measure of discrimination (b_j) *and* the effect of X_j on the propensity to report discrimination (c_j). It is perhaps worth noting that, even though the second-stage probit is run on only *part* of the sample, this induces no selection bias since the selection criterion (sex) is exogenous.

Inspection of Table 4 also reveals that, in the present case, the two-stage and maximum likelihood estimates should be numerically identical, since the two parts of the likelihood function can be maximized separately. The main reason for computing FIML estimates is thus to obtain correct standard errors for the underlying parameters of the model (especially for the c_js), as well as to establish a general "reference" model against which likelihood ratio tests can be performed.

Another issue in the structural model concerns identification. While all the earnings functions parameters are clearly identified, not all the underlying parameters in the probit part of the model are. In particular, note first that, as often happens, the variance term σ_v is not identified. To solve this problem, I adopt the convenient normalization $\sigma_v = \alpha$, which essentially determines the overall scale of the coefficients in the numerator (α, b, and c). Second, note that, even given the above normalization, the c_j parameters are only identified conditional on α. Intuitively, this implies the following. Suppose characteristic X_j had a large, positive effect on the propensity to report discrimination, (i.e., $b_j + (c_j/\alpha)$ is large), but that its contribution to statistically measured wage discrimination, b_j, was small (but positive). Then, supposing $c_j > 0$, the current model implies there is either: (*i*) a high relative weight given by individuals to nonstatistical evidence (low α); or (*ii*) a large positive effect of X_j on the level of nonstatistical evidence (high c_j). It cannot distinguish between these possibilities. Indeed this distinction is empirically irrelevant in determining the relative impact of the two types of evidence on reporting decisions. To resolve this ambiguity, I adopt the normalization $\alpha = 1$, which attributes (for convenience) all the difference in the relative effects of statistical vs. nonstatistical evidence on reporting to greater or lesser variation of these types of evidence across individuals. Thus, if, for example, nonstatistical evidence is the main determinant of the propensity to report, the c_j parameters will be large in absolute magnitude relative to the b_js when the model is estimated.

Finally, note that the basic model estimated in Section II can be seen as a special case of the structural model in Table 4. Specifically, if one imposes the restrictions $c_j = 0$, $\forall_j \neq 0$, and $\rho = 0$, the two models are equivalent. The parameters m and n in (6), are consistent estimates of c_C/σ_v and α/σ_v, respectively, only if these restrictions are satisfied. In the analysis below I shall present suggestive but not conclusive evidence that these restrictions do not hold in the current data.

B. *Results*

Full-information maximum-likelihood estimates of all the parameters in the structural model are reported in Table 5. They exhibit four main features. First, the female earnings function parameters, $a_j = \beta_j^F$, are essentially the same as those in Table 1; indeed, they differ from those results only because some categories of the independent variables are combined where no reports of discrimination occurred. Second, the parameters that estimate the systematic variation in statistical evidence across individuals $b_j = \beta_j^M - \beta_j^F$, ($j \geq 1$) are all small in magnitude relative to the c_js and, with one (apparently random) exception, insignificantly different from zero. Thus, the amount of systematic variation in statistically measured wage discrimination across women appears to be small in magnitude relative to the systematic variation in nonstatistical evidence. The main determinant of the propensity to report discrimination thus appears to be nonstatistical evidence.

Third, since the effect of characteristic X_j on the probability of reporting depends on the level of $b_j + c_j$ (see Table 4, recalling $\alpha = 1$ by assumption), it is clear from Table 5 that the two strongest determinants of the likelihood a woman will report discrimination are her age and education. More specifically, well-educated women (especially those with a college degree) are more likely to report discrimination, since $b_j + c_j$ is quite high for these groups, while older women (aged 55–76 in Canada; 45–76 in the United States) are less likely to report, since $b_j + c_j$ is quite negative in those categories. A convenient way to summarize this relationship arises if we note, from the a_j coefficients, that earnings are higher for younger, well-educated women than others. Thus the effects of age and education on earnings, together with their effects on total evidence of discrimination, induce a positive relationship between earnings and the propensity to report discrimination. Interestingly, this may help explain Leonard's finding that the OFCCP targets its affirmative action reviews disproportionally at firms employing highly skilled (nonclerical white-collar) women. While Leonard attributes this to a lower elasticity of labor supply (and thus less dissipation of the rents given to women), the current model suggests that the OFCCP may be responding to a greater likelihood of

TABLE 5—FIML ESTIMATES OF STRUCTURAL MODEL PARAMETERS

		Canada			United States		
		a	b	c	a	b	c
Intercept		1.285[b]	.167	−1.934[b]	.826[b]	.246[b]	−2.609[b]
		(.053)	(.089)	(.284)	(.086)	(.117)	(.574)
Age:	25–34	.111	−.028	.290	.057	.020	.328
		(.062)	(.091)	(.250)	(.070)	(.093)	(.397)
	35–44	.149[b]	.064	−.163	.061	.050	−.070
		(.066)	(.098)	(.332)	(.083)	(.123)	(.491)
	45–54	.018	.139	.036	−.129	.177	−1.066
		(.079)	(.111)	(.316)	(.098)	(.147)	(.622)
	55–76	−.030	.127	−.734	−.193	.099	−.709
		(.109)	(.136)	(.657)	(.106)	(.171)	(.785)
Education:	12 years[a]	.161[b]	−.014	.665[b]	.203[b]	−.042	.932
		(.054)	(.074)	(.284)	(.072)	(.086)	(.584)
	13–15 years	.245[b]	.048	1.004[b]	.270[b]	.025	.525
		(.067)	(.088)	(.306)	(.073)	(.090)	(.603)
	16 years	.540[b]	−.060	1.322[b]	.352[b]	.093	.466
		(.076)	(.113)	(.382)	(.087)	(.105)	(.679)
	17 years	.578[b]	−.071	1.724[b]	.606[b]	−.204	1.546[b]
		(.108)	(.128)	(.491)	(.089)	(.104)	(.616)
Tenure with Current Employer	2–3 years[a]	.036	−.088	.060	.063	.037	.082
		(.058)	(.085)	(.239)	(.068)	(.083)	(.362)
	4–5 years	.056	.083	−.097	.258[b]	−.209[b]	.437
		(.098)	(.119)	(.318)	(.065)	(.085)	(.358)
	6–10 years	.155[b]	−.030	−.162	.181[b]	.053	.067
		(.074)	(.098)	(.298)	(.072)	(.088)	(.389)
	11–20 years	.326[b]	−.119	−.381	.292[b]	−.087	.240
		(.066)	(.096)	(.405)	(.084)	(.103)	(.454)
	21 + years	.303[b]	−.055	−.327	.196	.121	−.073
		(.120)	(.142)	(.635)	(.107)	(.127)	(.942)
Experience in Total Labor Market	Experience in years				.007	.020	.014
					(.010)	(.013)	(.066)
	Experience squared				.00012	−.00053	.00004
					(.00024)	(.00028)	(.00184)
	Experience dummy	−.072	.120	.115			
		(.045)	(.062)	(.194)			
Trade School		.020	.031	−.023	−.089	.072	−.211
		(.091)	(.112)	(.324)	(.063)	(.074)	(.304)
Union Member		.114[b]	−.021	−.344	.191[b]	−.021	.020
		(.046)	(.061)	(.217)	(.054)	(.065)	(.271)
Physical Disability		.052	−.072	.157	−.099	.090	.601
		(.048)	(.065)	(.233)	(.069)	(.086)	(.363)
Estimate of:							
ρ			.118			.101	
			(.113)			(.157)	
σ_M			.413[b]			.375[b]	
			(.007)			(.009)	
σ_F			.367[b]			.352[b]	
			(.008)			(.010)	
Number of Observations			1106			992	
Log Likelihood			−701.30			−533.03	

Notes: Independent variables are the same as in Table 1 except where noted. The coefficients a_j, b_j, and c_j estimate the effect of characteristic X_j on (respectively) a woman's (log) hourly earnings, the level of statistically measured discrimination, and the amount of nonstatistical evidence of discrimination. The standard deviations of the earnings function disturbances, μ_i^M and μ_i^F, are given by σ_M and σ_F, respectively; ρ is the correlation coefficient between unobserved ability (μ_i^F) and the random component in the probit equation for reports of discrimination (v_i).

[a] Some categories used in the previous analyses have been combined with others because no individuals in the original categories reported discrimination.

[b] Denotes a coefficient significantly different from zero at the 95 percent level.

plausible reports of discrimination in such situations.

Finally, the variation in reports of discrimination with age, education and (hence) earnings is attributed by the model entirely to variations in nonstatistical evidence.[19] In fact, statistical evidence of discrimination appears to decrease somewhat with earnings (note the positive b_j coefficients for other women and the negative ones for well-educated women), but this effect is outweighed by a dramatic increase in nonstatistical evidence, $\sum_j c_j X_{ij}$, with earnings. This provides a simple explanation for the negative association between reports and measures found in the "naive" model, that is fully consistent with rational, honest reporting behavior by all women in the sample.[20]

Some examples of the kind of variation in nonstatistical evidence of discrimination with age and sex that the model indicates is occurring might be the following. If more-educated women are more likely to be in occupations that contain men, a larger number of them will be likely to have a readily accessible male against whom they can measure their labor market success, particularly in dimensions not measured in our data set. Thus they will have more nonstatistical evidence of discrimination without necessarily being more discriminated against according to the standard statistical measures.[21] Also, younger women may not yet have found a job in which they feel less discriminated against than in a randomly selected job, or may feel discriminated against because some employers' expectations have not adjusted to the changing labor market attachment of women over time. Finally, better-educated women may simply be more perceptive of the nonstatistical evidence that exists. While the present model cannot distinguish among these interpretations, it does indicate clearly that some underlying process of this kind is present in the data.

C. *Simulations: The Effect of Removing Statistical Evidence*

Further insights into the role of statistical evidence in individuals' decisions to report discrimination can be gained by calculating predicted reporting probabilities using the coefficients in Table 5, and comparing those probabilities to what would occur if women

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[19] Two possible objections to this interpretation are the following. First, as noted earlier, strategic motivations for reporting might be present. If so, the c_j coefficients will measure their incidence, as well as the incidence of nonstatistical evidence. Along these lines, one might interpret, for example, the age coefficients as follows: Younger women are more likely to report because reporting is an investment that might raise future wages, and younger women have a longer time horizon over which to reap these gains. Similar interpretations can be given to the other c_j coefficients. All such "strategic" interpretations of the current results, however, rely heavily on the assumption that reports of discrimination to a survey researcher have significant (positive *or* negative) economic consequences, which is far from obvious here. A second possible objection is based on *absolute* measures of discrimination. Since earnings increase with, say, education while percentage wage discrimination falls only slightly with education, it is easy to show that the absolute dollar amount of measured wage discrimination increases with education in these data. One might then be tempted to propose that this fact alone is sufficient to explain the positive effect of education on reporting found here. (A similar argument applies to the effect of age.) The main problem with this idea is, I believe, theoretical: if women's reporting decisions are truly an *inference* problem about the true level of discrimination, then given the well-known heteroskedasticity of absolute wages and homoskedasticity of log wages, it will be differences in log wages that form the appropriate test statistics for individuals to use in assessing whether true discrimination is positive or not. Another problem is that, when I estimated by FIML a model in which individuals base their reporting decisions on absolute discrimination levels and there is no systematic variation in the absolute level of nonstatistical evidence, this model (which can be derived as a special case of the structural model) is decisively rejected by a likelihood ratio test.

[20] It also helps to explain why the wage regressions which control for fewer characteristics in Table 3 generate strong negative relationships between reports and measures of discrimination. To see this, consider for simplicity the limiting case when no male characteristics are held constant. Then the amount of measured wage discrimination a woman faces is simply a constant (the average male's earnings) *minus* her own wage. The results in Table 5 make it less surprising that this measure of discrimination is negatively correlated with reports of discrimination.

[21] I am indebted to Gary Solon for this suggestion.

TABLE 6—SIMULATION RESULTS

	Canada			United States		
	Case 1	Case 2	Case 3	Case 1	Case 2	Case 3
1) Predicted (log) Hourly Wage	1.559	1.219	1.938	1.235	.828	1.590
2) Predicted Probability of Reporting Discrimination						
(a) Using All Evidence	.154	.015	.641	.099	.007	.345
(b) Using Nonstatistical Evidence Only	.102	.006	.603	.063	.001	.281
(c) Change (a–b)	.052	.009	.038	.036	.006	.064

Note: Case 1: Ages 35–44, 12 years school; Case 2: Ages 55–76, <12 years school; Ages 25–34, ≥17 years school.

totally discounted statistical evidence in their reporting decision. This is done in Table 6, which calculates women's predicted reporting probabilities using all available evidence (from 2(a) in Table 4), as well as reporting probabilities when statistical evidence is ignored by women (from the same term, but with $\Sigma_j b_j X_{ij}$ set equal to zero). The latter result can also be interpreted as the percentage of women who would continue to report discrimination even if an antidiscrimination policy which eliminated all measured wage discrimination was adopted.

Simulated reporting rates in Table 6 were calculated for three different cases. The first of these, Case 1, represents a woman with the median level of personal characteristics in both Canada and the Unites States.[22] The other two cases consider departures from this reference case that illustrate the combinations of personal characteristics that most tend to lower (Case 2), or raise (Case 3) the probability of reporting discrimination. Thus Case 2 considers an older woman with less education, and Case 3 the opposite. The three cases also represent women with very different earnings levels, as row 1 of Table 5 indicates, and can be thought of as representative of different cohorts in 1977.

Three main observations should be made on Table 6. First, Table 6 shows that the predicted reporting differences across age and education groups, discussed in the previous section, can be very large. Indeed, it is the fact that reports of discrimination are so strongly related to these characteristics while measured discrimination is not, that is responsible for this paper's main results concerning the relative importance of statistical evidence. Second, the largest absolute declines in reporting when statistical evidence is disregarded do not necessarily occur in the cases where statistical evidence of discrimination is strongest. This is apparent from a comparison of Cases 2 and 3, which reflects the following: Since the initial reporting rate of older, less-educated women based on *all* evidence is already very low, the subtraction of statistical evidence, even though it is greater for this group than for others, has little effect. Third, and most importantly, none of the declines in Table 6 are very large in magnitude. The largest absolute decline is for Case 3 in the United States, and amounts to 34.5 − 28.1 = 6.4 percentage points. This dramatically underscores the relative importance of factors *not* captured in economists' wage regressions in determining women's self-assessed levels of discrimination.[23]

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[22] With the exception that the values of *Experience* in Total Labor Market in the United States were arbitrarily chosen to represent "reasonable" levels for each of the three cases.

[23] This evidence also suggests that past court practice, in its exclusive reliance on nonwage evidence, may not have erred greatly in its choice of what evidence to use in assessing discrimination. It does not, however, imply that past court policy was effective and fair in providing remedies for discrimination. For example, if standards of proof are too rigorous and nonpecuniary court costs too high, then substantially fewer women would receive remedies than are experiencing discrimination, even if all available evidence was admissible in court.

D. *A Specification Test for the "Basic" Model*

Aside from generating the parameter estimates and predictions in Tables 5 and 6, the structural model has another function here. This is to serve as a reference model against which to measure the performance of the more naive model estimated in Section I, which yielded the "counterintuitive" result that the level of measured wage discrimination contributes negatively to a woman's assessment of her "true" discrimination level. Recalling that the naive model is equivalent to the structural model if $c_j = 0$ for $j \geq 1$, and $\rho = 0$, one can perform a likelihood ratio test of these restrictions by estimating a restricted model by FIML and comparing its performance to the structural model. When this was done, the value of the test statistic (-2 times the log of the likelihood ratio) for Canada and the United States, respectively, was 23.56 and 27.50. Using the χ^2 distribution with 18 and 19 degrees of freedom, respectively, this allows us to reject the naive model with 75 percent confidence in Canada and 90 percent confidence in the United States. These tests thus provide some weak evidence that the restrictions imposed in the naive model are invalid, and that the negative coefficient of reports on measures found there should not be interpreted as an estimate of the weight placed by women on measured wage discrimination in assessing discrimination at all. Rather, it appears possible that as suggested above, this empirical finding is the result of some underlying covariation between personal characteristics, statistical evidence of discrimination, and nonstatistical evidence, which may have important policy implications.

IV. Summary

This paper establishes a robust and puzzling result: the extent of conventionally measured wage discrimination against a woman is not positively related to the probability she will report discrimination in a confidential survey. Indeed, when the correlation between these two quantities is measured using a variety of specifications, it is always negative, although often insignificantly so. A coherent, formal explanation for this result is proposed in a "structural" model of reporting behavior. This model assumes that, besides measured wage discrimination, women have access to other sources of information about whether they are less well off than a comparable man, and that the presence of this nonstatistical information may vary systematically with a woman's characteristics.

When estimated, the structural model indicates that the level of nonstatistical evidence is a considerably more important determinant of self-assessed discrimination than conventional statistical evidence. This suggests that, if (as seems reasonable) U.S. courts place the same relative weight on statistical evidence in assessing discrimination as individuals themselves do, recent Supreme Court decisions making statistical evidence admissible are unlikely to generate a large increase in successful discrimination suits. The structural model also indicates that young, well-educated women behave as if they experience considerably more nonstatistical evidence of discrimination than others. This suggests that targetting of antidiscrimination policy toward such women (as observed by Leonard) might reflect a sensitivity to evidence of discrimination not captured by standard wage regression techniques.

APPENDIX:

DATA SET AND VARIABLE CONSTRUCTION

Canadian Data

The Canadian data are taken from the 1977 *Quality of Life Survey*, which was generated by the Social Change in Canada Project at York University. They were made available in the form of an SPSS data file by the Institute for Behavioral Research at York University. This survey is a sample of noninstitutionalized individuals, 18 years of age or older. Both the "national sample" and members of the "urban panel" from Toronto and Montreal were used. I refer to variables below by their SPSS variable name.

Because an analysis of selectivity bias was performed with the Canadian data, two samples drawn from the survey must be distinguished. One is the "overall" sample which was used to run reduced-form probit equations to estimate Heckman's λ. The second is the sample of full-time workers only, that was used in all the other analysis and is comparable to the *QES* sample. To be in the overall sample, an individual had to be white, not self-employed, working for pay, and under 77 years of age. Thus I excluded individuals who reported $Q27 = 3$, 10, 13, 13, 20, 21, or 26 (nonwhites); $Q60 = 1$ (self-employed); $Q64 = 0$ (working for free); or $RAGE \geq 77$. As well, individuals had to give usable responses (i.e., did not report "no answer" or "don't know") on all questions used in the analysis, described below. In order to be in the sample of full-time workers, an individual had to be in the overall sample *and* normally work more than 20 hours per week in total ($Q54 > 20$).

The respondent's log hourly wage on his or her main job was calculated from *Q54* (usual total weekly hours) and *Q64* (yearly salary at main job) for individuals who reported only one job, and from *Q57* (usual total weekly hours on main job) and *Q64* for individuals who had more than one job. Reported discrimination is taken from variables *Q254A–Q254E*, which code answers to the two questions described in the text.

Sources of the independent variables referred to in the paper are shown in parentheses following the variables:[24]

Age dummies (*RAGE*) Respondent's exact age on survey date.

Education dummies (*Q201*) Highest level of education completed.

Tenure dummies (*Q61*) "How long have you been with this company?"

Previous Experience dummy (*Q50BC; Q5859*) *Q50BC* gives an occupation-industry code for first full-time job after education completed. *Q5859* gives an occupation-industry code for current main job. Previous experience dummy equals one if these do not match; zero otherwise.

Trade School (*Q201*) Equals one if highest level of education completed is "technical training beyond secondary school."

Union (*Q63, Q110*) "Do you belong to a labor union?"

Physical Disability (*Q214*) "Do you have any longstanding health trouble or physical disability?"

Additional independent variables used only in the reduced-form probit for selection into the sample of full-time workers are

1) Marital Status Dummies (*Q222*) Two dummy variables were used—one for widowed, divorced or separated; one for married (left out category is "never married").

2) Number of Children living at home (*A1–A6, H1–H6*) Three variables were constructed, giving the number of children aged 0–5, 6–13, and 14+ living at home, respectively.

3) Exogenous Family Income (*Q157, Q158*) *Q157* gives total personal income of respondent from all sources before taxes in 1976. *Q158* gives total family income. Exogenous family income was calculated as *Q158–Q157*, using midpoints of the intervals provided. Responses in the top category for either question (100,000 and over) caused the respondent to be excluded from the analysis.

U.S. Data

The U.S. data are taken from the 1977 Cross Section data tape of the *Quality of Employment Survey*, distributed through the Inter-University Consortium for Political and Social Research. This survey is a sample of individuals 16 years of age and older who were working for pay for 20 or more hours per week. I shall refer to variables in the survey by their codebook number, which is a code between *V1* and *V887*.

To be in the sample for this paper, an individual had to be white, not self-employed, and under 77 years of age. Thus I excluded individuals who reported $V851 = 2$, 7, 8 or 9 (all nonwhites), or $V82 = 1$ (self-employed), or *V718* or $V727 = 97$ (born before 1901). As well, individuals had to give usable responses (i.e., did not report "don't know" or "no answer") on all questions used in the analysis, described below. The sample was restricted to individuals who reported annual salaries below $95,000.00, because actual salaries are not given above that level. As well, computational difficulties in the FIML model were avoided by restricting the sample to individuals with calculated log hourly wages between 0 and 3.9. This resulted in a loss of 9 men and 6 women.

The respondent's log hourly wage for regular work time on his or her main job was computed from *V394* and *V395*, which allow the respondent to stipulate both a rate of pay (*V394*) and the period to which it applies (hour, week, biweekly, monthly, annually, etc.–*V395*). Conversions to hourly rates were made on the basis of full-time, full year work (40 hours per week, 52 weeks per year). Reported discrimination is taken from *V621*, which is described in the text of the paper.

Sources of the independent variables used in the analysis follow:

Age dummies (*V718, V727*) Year of birth.

Education dummies (*V719, V728*) Highest grade of school or level of education completed.

Tenure dummies (*V91*) Years or months worked for present employer.

Experience, Experience squared (*V87*) Total years worked for pay since age 16.

Trade School (*V720, V729*) "Have you ever gone to a trade school?"

Union (*V561*) "In your present job, do you belong to a union or employees' association similar to a union?"

Physical Disability (*V240*) "Do you have anything you regard as a physical or nervous condition that limits the amount of work you do?"

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[24] The questions for *Tenure*, *Previous Experience*, and *Union* were not asked of all individuals who did not work. For the purposes of the selectivity bias correction only, values were assigned to these individuals as follows. All nonworkers were assigned less than one year of tenure. The previous experience dummy was set equal to one if the individual ever had a full-time job, and zero otherwise. Nonworkers were classified as nonunion unless they were currently unemployed and reported belonging to a union.

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The Optimal Use of Nonmonetary Sanctions as a Deterrent

By STEVEN SHAVELL*

A theoretical model of deterrence is studied in which the imposition of nonmonetary (as opposed to monetary) sanctions is socially costly. It is therefore desirable that the system of sanctions be designed so that sanctions are imposed infrequently. If courts possess perfect information, the optimal system is such that sanctions are never imposed — all who can be deterred will be — but, realistically, courts' information will be imperfect and sanctions will be imposed.

This article studies the optimal use of nonmonetary sanctions (such as probationary restraints on conduct, imprisonment, the death penalty) in a simple model in which the threat of sanctions may deter parties from committing contemplated acts. An important assumption of the model is that the imposition of nonmonetary sanctions is socially costly. The motivation for this assumption is that the imposition of nonmonetary sanctions is often associated with direct claims on goods and services (as with the operation of the prison system) and, in contrast to the case with monetary sanctions, results in disutility to punished parties that is not balanced in any automatic way by additions to the utility of other parties.

Social welfare is defined in the model to be the benefits parties obtain from committing acts less the harm done, the costs of apprehending parties, and the costs of imposing sanctions.[1] The social problem is to choose a set of sanctions and the probability of apprehension in order to maximize social welfare. As will be emphasized, the solution to this problem will involve design of a system of sanctions under which sanctions are actually imposed infrequently—since their imposition is assumed to be socially costly.

The social problem is first examined under the hypothetical assumption that *courts*[2] *can obtain perfect information about parties who have been apprehended.* With perfect information, courts will be able to recognize the two situations in which it will not be optimal to impose sanctions. One is where a party's act, such as speeding in an emergency, was socially desirable. A policy of imposing sanctions here will not be optimal because it might discourage socially desirable acts and, even if not, will result in pointless imposition of costly sanctions. The other situation where imposing sanctions will not be optimal is where, although a party's act was undesirable, it could not possibly have been deterred given the probability of apprehension. (Deterrence may have been impossible given the probability of apprehension because the disutility of sanctions is bounded.) Consider, for instance, the act of killing in the heat of passion or stealing a large amount where the chances of discovery are low. If deterrence of an act was impossible, clearly it will not be optimal to impose a sanction.

The remaining situation that courts will be able to recognize, assuming that they can obtain perfect information, is where a party's act was undesirable and could have been deterred by a sufficiently high sanction given the probability of apprehension. In this type of situation, it will be best for courts to adopt the policy of imposing sanctions large enough to deter, for then—since deterrence

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[1] Here and below, the word "sanctions" will mean nonmonetary sanctions.

[2] The term "courts" refers to the social authority responsible for deciding upon sanctions.

will be accomplished—there will never be an occasion in which a sanction is applied. In other words, any act that can be deterred given the probability of apprehension will be deterred without the actual imposition of costly sanctions. Suppose, for instance, that a party would obtain a benefit of 50 from committing an undesirable act, that the probability of his being apprehended is 10 percent, and that the disutility of possible sanctions can range as high as 1,000 (corresponding, say, to the death penalty). Then the party could and would be deterred under the optimal sanctioning policy; a sanction exceeding 500 would deter him. Moreover, note here, and generally, that the size of the optimal sanction is not uniquely determined. The sanction could as well be extremely high (1,000) or barely sufficient to deter (just over 500), since an extremely high sanction is no more costly than a lower one if neither will be imposed. In addition, observe that the size of the optimal sanction does not depend on the harmfulness of the act (whether the act does great or only moderate harm, the optimal sanction will still be any sanction exceeding 500), presuming that the harmfulness of the act is substantial enough to make it undesirable. The optimal sanction will, however, be affected by the probability of apprehension and by the benefits a party would derive from an act, because the expected sanction must be high enough to offset the benefits. Finally, the optimal probability of apprehension will reflect a balancing of two factors: the greater is the probability, the more parties who can possibly be deterred, but the larger are the expenses of policing behavior.

The social problem is then reconsidered under the realistic assumption that *courts cannot obtain perfect information about parties*. Thus courts will not be able to employ sanctions as just described; they will make various "errors" relative to the situation under perfect information, resulting in the discouragement of some socially desirable acts and in the actual imposition of sanctions. Courts may turn out to impose sanctions for three reasons: they may fail to recognize that an act was really socially desirable; they may fail to recognize that an undesirable act really could not have been deterred; they may fail to recognize how high a sanction was really necessary to deter an undesirable act that could have been deterred. Furthermore, unlike in the case where courts' information is perfect, the optimal magnitude of sanctions is uniquely determined[3]—it represents an appropriate compromise between achieving greater deterrence and the social costs borne due to the actual imposition of sanctions—and it does depend on the magnitude of harm done. The optimal probability of apprehension will reflect factors similar to those in the case where courts' information is perfect.[4]

I. The Model

Parties decide whether to commit harmful acts from which they would derive benefits.

[3] To illustrate, suppose the numerical example is modified as follows. There is another party who would obtain benefits of 150 from committing the undesirable act and thus who could not be deterred even by the maximum sanction of 1,000 (since the maximum expected sanction is 100). Suppose also that this second party cannot be distinguished from the first by courts—the sense in which courts' information is imperfect. Hence, courts must apply the same sanction to each party. It follows that a sanction of 500, but not one above 500, will be optimal: a sanction of just 500 will deter the first party; raising the sanction above 500 would not deter the second party but would lower social welfare because it would increase the expected social cost due to imposition of sanctions on the second party. (While the probability of apprehension was assumed to be fixed at 10 percent in this example, the probability would, of course, be optimally determined in the solution to a completely specified example; see Sections II and III.)

[4] The results described in this and the previous paragraph have not been discussed in the literature on deterrence to my knowledge; see Gary Becker (1968), R. Carr-Hill and Nicholas Stern (1979), A. Mitchell Polinsky and myself (1984), and references cited therein. In that literature the analysis has sometimes been carried out mainly at the level of the aggregate number of offenses (as in Becker); and where the analysis has been conducted at the level of the individual (as in Polinsky's and my article), it has not focused on the information the courts are able to obtain about an apprehended individual's benefits and the harmfulness of his act, and has usually been concerned with monetary sanctions and has implicitly assumed that such sanctions are socially costless to impose.

Let[5] b = benefit to a party from committing a harmful act; $0 \leq b \leq \bar{b}$; and h = harm resulting from an act; $0 \leq h \leq \bar{h}$.

A particular party is identified by b and h, and the distribution of parties by type is described by $f(b, h)$ = probability density of b and h; the function f is assumed to be known by courts. If a party commits an act, he might be apprehended and suffer a sanction. Let p = probability that a party who commits an act is apprehended and suffers a sanction; and s = sanction; $0 \leq s \leq \bar{s}$.

The level of s is determined by a sanctioning function, that is, a function of the variables that courts can observe (as discussed below). The utility of a party who commits an act will be b if he is not sanctioned and $b - s$ if he is; if he does not commit an act, his utility will be 0. Therefore, he will commit an act if[6]

$$(1) \qquad b > ps.$$

Because s is bounded by $\bar{s}$, it may be impossible to deter a party from committing an act. This will be true if

$$(2) \qquad b > p\bar{s},$$

and such a party will be referred to as undeterrable given p.

Social welfare is defined to be the benefits parties obtain from their acts,[7] less the harm done, the social costs due to the imposition of sanctions, and the costs of apprehending parties. In particular, suppose that σ = weight for calculating social costs from imposition of sanctions; σs is the social cost if the sanction imposed is s; $\sigma > 0$;[8] and $c(p)$ = social cost of maintaining the probability of apprehension at p; $c(p) > 0$; $c'(p) > 0$; $c''(p) > 0$.

Social welfare thus equals

$$(3) \qquad \int\int (b - h - p\sigma s) f(b, h)\, db\, dh - c(p),$$

where the integration is performed over the set of parties who commit the act as determined by (1).

The social problem is to choose the sanctioning function and the probability p to maximize social welfare; p will be assumed to be the same for all parties.[9] This choice will be called the optimal system of deterrence and will be denoted s^* and p^*. It will be assumed that $p^* > 0$; otherwise the social problem is without interest.

The first-best behavior of parties—that which would maximize social welfare if parties' behavior could be commanded—is for parties to commit acts if and only if

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[5] The benefits and (see below) the sanction are assumed to be bounded because, as is well known, the usual axioms of expected utility theory imply the boundedness of utility; see, for instance, Kenneth Arrow (1971).

[6] If $b = ps$, the party will be indifferent between committing the act and not; but in order to avoid having to make tedious qualifications, I adopt the convention that the individual will not commit the act in this case, and I adopt similar conventions below without further comment.

[7] In my paper (1985b, a previous version of this paper), parties' benefits were multiplied by a weight β that could be less than 1, so as to allow for society to discount parties' benefits. (Some might find the assumption of such discounting appealing where the source of a party's utility is the disutility experienced by a victim.) But the qualitative nature of the conclusions that were obtained there did not depend on whether β was less than 1, and were the same as the conclusions that will be reached below.

[8] The weight σ will exceed 1 if it measures both the resource costs of imposing sanctions and the disutility suffered by sanctioned parties.

[9] In fact, of course, p can be varied to some degree according to the characteristics b and h of parties; p will be constant only over subpopulations. Thus, if the reader wants, he may reinterpret the assumption that p is the same for all parties: he may say that for each subpopulation of parties (described by a density f) among whom p must be uniform, the social problem will be as posed here. In any case, the reader should note that there are two reasons why p will not vary in a completely independent manner with b and h. First, it will often be inefficient for an enforcement authority to be assigned responsibility to apprehend only one type of party. (It would be inefficient to have a policeman on the beat pursue only one type of lawbreaker; it will be efficient to have him pursue any lawbreaker whom he has a good opportunity to apprehend.) In other words, it is natural to assume that enforcement effort will often affect different types of parties in similar ways. Second, the ability of enforcement authorities to tailor their efforts to particular types of parties will be limited by authorities' ability to distinguish among types of parties *ex ante*.

$$b > h. \tag{4}$$

II. The Optimal System of Deterrence Where Courts Possess Perfect Information

In this case, courts can determine the benefits b and the harm h, so the sanction s may be a direct function $s(b, h)$ of b and h. Thus, we have

PROPOSITION 1: *If courts are able to obtain perfect information about parties who are apprehended, then under the optimal system of deterrence,*

(a) *Parties whose acts are undesirable and who can be deterred (given the probability of apprehension) will be deterred by the threat of a positive sanction—of at least* b/p^*; *such sanctions will equal the maximum possible sanction,* $\bar{s}$, *for some parties:*

(b) *Parties whose acts are undesirable and who cannot be deterred, and also parties whose acts are desirable, will face no sanction and will commit their acts.*

(c) *Sanctions will therefore never actually be imposed.*

(d) *The optimal probability of apprehension is such that the marginal cost of raising the probability equals the reduction in harm net of benefits of additionally deterred parties.*

The proof is given in the Appendix. Several characteristics of the optimal system of deterrence may now be noted.

(*i*) *Relation of the optimal sanction to h, b, and p*.* An increase in h has no necessary effect on $s^*(b, h)$ within the area where parties' acts are undesirable and can be deterred since $s^*(b, h)$ need only satisfy $b \leqq p^*s^*(b, h)$ there; an increase in h could, however, make a desirable act undesirable and thus raise $s^*(b, h)$ from 0 to a positive level. An increase in b increases the minimum $s^*(b, h)$ needed to deter within the area of undesirable deterrable acts, namely, b/p^*; but an increase in b could make a deterrable act undeterrable and thus reduce $s^*(b, h)$ from a positive level to 0. An increase in p^* reduces the minimum $s^*(b, h)$ needed to deter in the area of undesirable deterrable acts, but because it enlarges this area, it can result in an increase in $s^*(b, h)$ from 0 to a positive level.

(*ii*) *Perfect information about b and h is socially valuable.* This is clear since $s^*(b, h)$ depends on both b and h in a nontrivial way.

(*iii*) *Comparison with the first-best situation.* The situation under the optimal system of deterrence is inferior to the first-best situation because undesirable acts that cannot be deterred are committed and because the expense $c(p^*)$ is incurred.

(*iv*) *Example.* To illustrate the optimal system of deterrence, consider the following discrete example in which there are three levels of benefits for each of three levels of harm:[10] parties of type A enjoy benefits of 50 from committing acts, those of type B enjoy benefits of 100, and those of type C benefits of 150; there are 10 A's, 1 B, and 10 C's for each level of harm; the levels of harm are 80, 300, and 900. In addition, $\bar{s} = 1{,}000$, $\sigma = 1$, and $c(p) = 1{,}000p$ for $p \leqq .12$ and $c(p) = 1{,}000{,}000p$ for $p > .12$. In this example, it is easy to verify that deterring the C's would not be optimal since that would require p to be at least .15,[11] but being able to deter A's and B's will be worthwhile since this will require p to be only .1; and $p^* = .1$ since it will clearly not be optimal for p to be any larger than the minimum necessary to deter the A's and B's. Hence, when $h = 80$, s^* will be at least 500 for A's, so they will be deterred, and s^* will be 0 for B's and C's. When $h = 300$ or 900, s^* will be at least 500 for A's and s^* will be 1,000 for B's, so A's and B's will be deterred, and s^* will be 0 for C's. Note that the outcome will differ from the first-best outcome since C's will commit acts when the harm is 300 and 900, and since the expense $c(.1) = 100$ will be incurred.

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[10] It will be obvious that nothing rests on the discrete nature of this example; the same example will be reconsidered in the next section.

[11] If p were .15 rather than .1, the probability necessary to deter the B's, costs of apprehension would rise by $c(.15) - c(.1) = 31{,}200 - 1{,}000 = 30{,}200$. This amount clearly exceeds the harm net of benefits done by C's, so it cannot be optimal for p to equal .15.

III. The Optimal System of Deterrence Where Courts' Information is Imperfect

Now assume that while the courts can determine h, they cannot determine b; in particular, let $r =$ imperfect indicator of b observed by the courts; $0 \leqq r \leqq \bar{r}$.[12] Then social welfare may be written as

$$(5)\quad \int_0^{\bar{h}}\int_0^{\bar{r}}\int_{ps}^{\bar{b}}(b-h-p\sigma s)f(b|r,h) \times f(r|h)f(h)\,db\,dr\,dh - c(p),$$

where $f(b|r,h)$ and $f(r|h)$ are conditional probability densities, $f(h)$ is the unconditional density of h, and s is understood to be a function $s(r,h)$.[13] The solution to the social problem of maximization of (5) over sanctioning functions s and over p is described by

PROPOSITION 2: *If courts cannot obtain perfect information about apprehended parties, then under the optimal system of deterrence,*

(a) *There will generally be parties who commit acts and who are sanctioned. These parties will generally include both those whose acts are and are not desirable, and both those who are and are not deterrable.*

(b) *Also, some parties will generally be discouraged from committing desirable acts.*

(c) *The optimal sanction must be the maximum, $\bar{s}$, for some parties. If the optimal sanction is not 0 or $\bar{s}$, it will be such that the marginal social cost of raising sanctions, in terms of sanctions actually imposed, equals the net marginal social benefits due to deterrence of additional parties.*

(d) *The optimal probability of apprehension is such that the marginal cost of raising the probability plus the cost of imposing sanctions more frequently equals the reduction in harm net of benefits of additionally deterred parties.*

The proof is given in the Appendix. Characteristics of the optimal system of deterrence will now be discussed.

(*i*) *Relation of the optimal sanction to h, r, and p^*.* An increase in h causes $s^*(r,h)$ to rise, other things equal. To show this, note that the first-order condition (11) (see the Appendix) determining s^* is of the form $g(s^*,h)=0$.[14] Implicitly differentiating this with respect to h, we obtain $s^{*\prime}(h) = -g_h(s^*,h)/g_s(s^*,h)$. Now $g_s(s^*,h)<0$ (the second-order condition for optimality of s^*) and, assuming that the conditional density f does not change with h (to isolate the effect of a change in h), we have $g_h(s^*,h) = pf(ps|r,h) > 0$, so that $s^{*\prime}(h) > 0$ as claimed. The explanation for this is that if h increases, the marginal social cost of imposing sanctions is unaffected, but the marginal benefits of deterrence are increased.

Suppose that an increase in r indicates a rightward shift in the conditional distribution of b, that is, $\mathrm{Prob}[b \geqq x|r,h]$ rises with r (where the probability is less than 1). Then writing (11) in the form $g(s^*,r)=0$ and proceeding analogously to the previous paragraph, we see that the sign of $s^{*\prime}(r)$ equals the sign of $g_r(s^*,r)$. Now $g_r(s^*,r) = -p^*\sigma d[\mathrm{Prob}[b \geqq p^*s^*|r,h]]/dr + p(h+p^*\sigma s^* - p^*s^*)f_r(p^*s^*r|,h)$. By assumption, the derivative of the probability is positive, so the first term is negative; and from (11), $p(h+p^*\sigma s^*-p^*s^*)>0$. Hence, certainly if $f_r(p^*s^*r|,h)<0$, g and thus s^* decrease with r; but if $f_r(p^*s^*|r,h)$ is sufficiently high, s^* could increase. The explanation for the ambiguity of the effect of an increase in r is straightforward: when the conditional probability distribution of b

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[12] We can imagine that $r = r(b,h,\theta)$, where θ is a random variable, but we will not need to take the generation of r into explicit account.

[13] The lower limit of integration in (5) is ps because the assumption is that parties commit acts if $b > ps = ps(r,h)$. This means that it has been implicitly assumed that parties can predict what a court's observation r will be. It would be more realistic to assume that parties can only imperfectly predict what a court's observation r will be; but to assume that would complicate the expressions below without changing the essential nature of the results.

[14] It is assumed here and elsewhere in (*i*) that $s^*(r,h)$ is in $(0,\bar{s})$ and is thus determined by the first-order condition; otherwise, as $s^*(r,h)$ will be a corner solution, it will not change with small variations in h, r, and p^* (and the way it will change with large variations is similar to what was described in (*i*) following Proposition 1).

shifts to the right, the marginal social cost of sanctions increases (since more parties commit acts, more suffer sanctions); but the marginal social benefits may increase or decrease (because the density of parties just deterred may increase or decrease).

A small increase in p^* will lower $s^*(r, h)$ proportionately. This is immediately evident from the form of (9) (see the Appendix). Since p and s appear in (9) only as a product, it is clear that if p^* rises, lowering s^* so as to maintain $p^*s^*(r, h)$ constant must continue to maximize (9).[15]

(*ii*) *Imperfect information about parties' benefits is socially valuable*. The imperfect information r is socially valuable because given any h, $s^*(r, h)$ depends on the conditional density function $f(\cdot|r, h)$ in a nontrivial way.

(*iii*) *Comparison with the situation where courts possess perfect information*. The situation here differs because some parties suffer sanctions and because some parties whose acts are desirable may be discouraged from committing them.

(*iv*) *Example*. To illustrate the optimal system of deterrence, reconsider the example from the last section assuming that courts can observe h only; since they have no information about b, the sanction must be a function of h alone. The reader can verify that, as before, $p^* = .1$. The reader can easily see that for an h, p^*s^* must equal either 0, so that no parties are deterred; or 50, so A's only are deterred; or 100, so that A's and B's are deterred; that is, s^* must equal either 0, 500, or 1,000.[16] He can then verify that when $h = 80$, $s^* = 0$, so that A's, B's, and C's will commit acts;[17] when $h = 300$, $s^* = 500$, so that B's and C's will commit acts; and when $h = 900$, $s^* = 1{,}000$, so that C's only will commit acts. Comparing the outcome with that where courts had perfect information, we see that when $h = 80$, A's commit acts here but A's did not before (the problem here, of course, being inability to identify A's). When $h = 300$, B's commit acts here but did not before and B's and C's are sanctioned here but were not before; and when $h = 900$, C's are sanctioned here but were not before.

IV. Concluding Comments

(a) The model could be extended in the following ways.[18]

(*i*) An act could be associated with a probability distribution of harm (rather than with a single and certain level of harm). If so, it would be natural to assume that courts are unable to obtain perfect information about the probability distribution, for while courts can usually determine directly the harm actually done, they may lack important information about the harm that might have been done (as in an attempted crime).

(*ii*) A party's benefits could be allowed to depend on harm done. This assumption is often realistic since a party's object is often to do harm (as in murder or theft) and would furnish an indirect reason why the sanction should rise with harm: the higher the harm, the higher the party's benefits and thus the higher the sanction probably needed to deter him.

(*iii*) A party could be allowed to choose among a set of harmful acts. This would allow study of the issue of "marginal deterrence": discouraging a party who is *not* deterred from committing an undesirable type of act (kidnapping) from doing greater harm while committing the act (killing his victim) by making the level of the sanction depend on the level of harm done.

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[15] Recall, however, that we are assuming the change in p^* is small enough that s^* is still determined by the first-order condition.

[16] Were s to equal any other amount, deterrence would not be improved but more costs due to imposition of sanctions would be borne. For instance, if $s = 600$, then A's would be deterred, just as they would if $s = 500$, but B's and C's who were apprehended would suffer sanctions of 600 rather than 500.

[17] To see that $s^* = 0$ when $h = 80$, observe that if $s = 0$, the change in social welfare (parties' benefits less the harm from acts and costs of imposing sanctions) will equal $(10 \times 50 + 100 + 10 \times 150) - (21 \times 80) = 420$; whereas if $s = 500$, the change in welfare will equal $(100 + 10 \times 150) - (11 \times 80) - (11 \times 50) = 170$; and if $s = 1{,}000$, the change in welfare will equal $(10 \times 150) - (10 \times 80) - (10 \times 100) = -300$. That s^* will be as claimed when h is 300 and 900 can be similarly verified.

[18] The extensions are discussed informally in my article (1985a).

(b) The main points regarding the optimal sanction shed light on the principles and doctrines of criminal law.[19] For instance, the importance given to "intent" in the criminal law may further the purposes of deterrence, for intent can be argued to be a rough proxy for a variety of factors (including the magnitude of the benefits a party derives from committing an act and the expected harmfulness of an act) that raise the optimal sanction. The results concerning the optimal sanction also obviously suggest the rationality of not punishing those who probably cannot be deterred (the insane or the coerced) or those whose acts were not undesirable (those who kill in self-defense).

(c) The optimal use of nonmonetary sanctions may be contrasted with the optimal use of monetary sanctions—Pigouvian taxes—in the classic model of harmful externalities. In that model, of course, if parties pay for harm done, a first-best outcome results, and the only information required by the social authority is the magnitude of the harm. The chief reason that the first-best outcome results without the authority's needing more information is that *imposition of taxes is implicitly assumed to be socially costless*; since the fact that parties for whom benefits exceed harm *pay* taxes does not itself reduce social welfare, there is no need for a social authority to ascertain parties' benefits.

(d) While in the present article, it was assumed that the form of sanctions was nonmonetary, the social decision whether to employ such sanctions rather than only monetary sanctions could also have been studied. Were this done, the presumed conclusion would be that nonmonetary sanctions would not be optimal to use unless the socially costless (or at any rate less costly) monetary sanctions could not adequately deter parties. That in turn would be more likely to be the case where the harm parties might do is great in relation to their assets and where it would be difficult to identify or apprehend parties who do harm.[20]

APPENDIX

PROOF OF PROPOSITION 1:

(a) The claim is that if $b \leq h$ and $b \leqq p\bar{s}$, then $s^*(b,h)$ is any s satisfying $b \leqq ps$. This is obviously so, for such an s will prevent a decline in social welfare (see (3)) of $h - b$ and not involve the actual imposition of sanctions since the party will be deterred.

To show that $s^*(b,h) = \bar{s}$ for some parties, assume otherwise. Then it would be possible to lower p^* slightly and raise all positive $s^*(b,h)$ so as to maintain $p^*s^*(b,h)$ constant. This would result in the same behavior of parties, so that social welfare would not change on that account, but welfare would rise since $c(p)$ would fall. Hence, social welfare could not have been maximized, a contradiction.

(b) The first claim is that if $b \leq h$ and $b > p\bar{s}$, then $s^*(b,h) = 0$. This is clearly so, since imposing a positive sanction would not deter the party but would lower social welfare by $p\sigma s$. The other claim is that if $b > h$, then $s^*(b,h) = 0$. This is true because with $s^* = 0$, the party will commit his act, increasing social welfare by $b - h > 0$; whereas with a positive sanction, social welfare would rise by only $b - h - p\sigma s$ if the party was not deterred and would not rise at all if he was deterred.

(c) This is immediate from (a) and (b).

(d) From (a) and (b), we know that social welfare can be written

$$\text{(A1)} \quad \int_0^{\bar{b}} \int_h^{\bar{b}} (b-h) f(b,h)\, db\, dh - \int_{p\bar{s}}^{\bar{h}} \int_{p\bar{s}}^{\min(h,\bar{b})} (h-b) f(b,h)\, db\, dh - c(p).$$

The first term reflects the commission of socially desirable acts and the second, undesirable acts. The derivative of (A1) with respect to p is

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[19] See generally Richard Posner (1985) and my paper (1985a).

[20] This is a theme discussed informally in Posner and my paper (1985a); closely related points are made in Becker (pp. 190–93) and Polinsky and Shavell (p. 95).

$$\text{(A2)}\quad \bar{s}\int_{p\bar{s}}^{\bar{h}}(h-p\bar{s})f(p\bar{s},h)\,dh + \bar{s}\int_{p\bar{s}}^{p\bar{s}}(p\bar{s}-b)f(b,p\bar{s})\,db - c'(p) = \bar{s}\int_{p\bar{s}}^{\bar{h}}(h-p\bar{s})f(p\bar{s},h)\,dh - c'(p),$$

from which it follows that p^* is determined by

$$\text{(A3)}\quad c'(p) = \bar{s}\int_{p\bar{s}}^{\bar{h}}(h-p\bar{s})f(p\bar{s},h)\,dh.$$

The left-hand side is the marginal cost of raising p and the right-hand side is the marginal gain due to deterring additional parties.

PROOF OF PROPOSITION 2:

(a) and (b) are obviously true since the sanction $s^*(r,h)$ is not a function of b.

(c) The reason that $s^*(r,h)$ must equal $\bar{s}$ for some parties is essentially the reason given in the proof to Proposition 1: if not, p^* could be lowered slightly and all positive $s^*(r,h)$ could be raised so as to maintain $p^*s^*(r,h)$ constant. Then the integral in (5) would not change since p and s enter only as a product, yet $c(p)$ would fall, a contradiction.

To derive the first-order condition determining $s^*(r,h)$ when it is in $(0,\bar{s})$, note from (5) that $s^*(r,h)$ must maximize

$$\text{(A4)}\quad \int_{ps}^{\bar{b}}(b-h-p\sigma s)f(b|r,h)\,db$$

over s. The derivative of this expression with respect to s equals

$$\text{(A5)}\quad -p\sigma\text{Prob}[b \geqq ps|r,h] + (h+p\sigma s-ps)pf(ps|r,h),$$

so that if $s^*(r,h)$ is in $(0,\bar{s})$, it is determined by

$$\text{(A6)}\quad p\sigma\text{Prob}[b \geqq ps|r,h] = (h+p\sigma s-ps)pf(ps|r,h).$$

The left-hand side of (A6) is the marginal cost of increasing s—the expected marginal social cost $p\sigma$ of increasing the sanction per party who commits an act multiplied by the conditional likelihood $\text{Prob}[b \geqq ps|r,h]$ that a party will do this; and the right-hand side is the marginal benefit of increasing s—the expected benefit per party just deterred multiplied by the rate of increase in these parties.

(d) For a given r and h, let $w(s,p)$ denote the value of (A4) given s and p, and write $s^*=s^*(p)$ to show the dependence of s^* on p (r and h are suppressed in the notation for now). We claim first that if s^* is determined by (A6), then $dw(s^*(p),p)/dp = 0$. To see this, observe that $dw(s^*(p),p)/dp = w_s s^{*\prime}(p) + w_p = w_p$ since $w_s = 0$ (this is (A6)); and w_p is easily verified to be a positive multiple of w_s, so that $w_p = 0$. If s^* is not determined by (A6), one possibility is that $s^*=0$ and that the nonnegativity constraint is binding. In this case as $s^{*\prime}(p) = 0$, $dw(s^*(p),p)/dp = w_p$; but as w equals $\int_0^{\bar{b}}(b-h)f(b|r,h)\,db$, we have $w_p = 0$, so that the derivative is again 0. The remaining possibility is that $s^* = \bar{s}$ and the constraint $s \leqq \bar{s}$ is binding. In this case, as $s^{*\prime}(p)=0$, we again know that $dw(s^*(p),p)/dp = w_p$; and as $w = \int_{p\bar{s}}^{\bar{b}}(b-h-p\sigma\bar{s})f(b|r,h)\,db$, $w_p = -\sigma\bar{s}\,\text{Prob}[b \geqq p\bar{s}|r,h] + \bar{s}(h+p\sigma\bar{s}-p\bar{s})f(p\bar{s}|r,h)$. It follows from this, the fact that the integral in (5) may be rewritten as $\int_0^{\bar{h}}\int_0^{\bar{r}} w(s^*(p,r,h),p;r,h)f(r|h)f(h)\,dr\,dh$, and the fact that $w_p = 0$ on the boundary of the set $Z(p)$—defined to be the set of r and h where $s \leqq \bar{s}$ is binding—that p^* is determined by

$$\text{(A7)}\quad \sigma\bar{s}\int\int_{(r,h)\varepsilon Z(p)}\text{Prob}[b \geqq p\bar{s}|r,h] + c'(p) = \bar{s}\int\int_{(r,h)\varepsilon Z(p)}(h+p\sigma\bar{s}-p\bar{s})f(p\bar{s}|r,h) \times f(r|h)f(h)\,dr\,dh.$$

The left-hand side of (A7) is the marginal social cost of increasing p, comprising both additional sanctions suffered (since more parties are apprehended) and the direct expense of raising p. (By comparison, in (A3) the marginal cost equaled only $c'(p)$, for no parties actually suffered sanctions.) The right-hand side is the marginal benefits in terms of increased deterrence.

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If *Homo Economicus* Could Choose His Own Utility Function, Would He Want One with a Conscience?

By ROBERT H. FRANK*

A blush may reveal a lie and cause great embarrassment at the moment. But in situations that require trust, there can be great advantage in being known to be a blusher. This paper develops a model in which tastes are determined endogenously for their capacity to help solve the so-called precommitment problem. The tastes that emerge are very different from those assumed in conventional models of rational choice.

The rational choice model takes tastes as given,[1] and assumes that people pursue self-interest. The model performs well much of the time, yet apparent contradictions abound. Travelers on interstate highways leave tips for waitresses they will never see again. Participants in bloody family feuds seek revenge even at ruinous cost to themselves. People walk away from profitable transactions whose terms they believe to be "unfair." The British spend vast sums to defend the desolate Falklands, even though they have little empire left against which to deter future aggression. In these and countless other ways, people do not seem to be maximizing utility functions of the usual sort.

In this paper, I investigate the familiar theme that seemingly irrational behavior can sometimes be explained without departing from the utility-maximization framework. Efforts along these lines have generally focused on a more careful assessment of the constraints people face. The alternative I explore here is to suggest that tastes differ in systematic ways from those portrayed in standard economic models. Instead of treating tastes as a datum, I retreat a step and ask, "What kind of tastes would maximize the attainment of selfish objectives?" This is essentially the behavioral biologist's approach (W. D. Hamilton, 1964; E. O. Wilson, 1975, 1978; Richard Dawkins, 1976; Robert Trivers, 1985). It treats tastes not as ends in themselves, but as means for attaining important material objectives.[2]

My focus is a specific example in which people have opportunities to cheat without possibility of reprisal. This example illustrates that it will sometimes be in a selfish person's interest to have a utility function that predisposes him not to cheat, even when he is *certain* he would not be caught. The example also suggests other specific elements of the utility function that might help people resolve important market failures and bargaining problems. The common feature of these problems is that their solution requires people to make *ex ante* commitments to behave in ways that will not be self-serving *ex post*.

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[1] Gary Becker and George Stigler (1977) provide the clearest statement of the modern economists' hands-off approach to tastes. For three recent exceptions that are closely related to the theme of this paper, see Thomas Schelling (1978), George Akerlof (1983), and Jack Hirshleifer (1984). See also Hirshleifer (1977; 1978a,b), Philip Coelho (1985), Paul Rubin and Chris Paul (1979), Rubin (1982), and myself (1985a,b; 1986, 1988).

[2] In the biologist's view, tastes are no different from other characteristics. All are selected for their capacity to promote survival and reproduction. A wolf who "likes" to stand up to intruders runs the risk of serious injury. But because the animal is known to have that preference, it may do much better, on the average, than one who is known to prefer passive retreat. If so, the success conferred by this taste will lead to an increase in its frequency in the gene pool.

I. The Commitment Problem

Thomas Schelling (1960) provides a vivid illustration of this class of problems. He describes a kidnapper who suddenly gets cold feet. He wants to set his victim free, but is afraid he will go to the police. In return for his freedom, the victim gladly promises not to do so. The problem, of course, is that both realize that it will no longer be in his interest to keep this promise once he is free. And so the kidnapper reluctantly concludes he must kill his victim.

Schelling suggests the following way out of the dilemma: "If the victim has commited an offense whose disclosure would lead to blackmail, he may confess it; if not, he might commit one in the presence of his captor, to create a bond that will ensure his silence" (1960, pp. 43–44). The blackmailable act serves here as a "commitment device," something that provides the victim with an incentive to keep his promise. The need for such devices arises frequently in the course of economic interaction.

II. The Costs of Monitoring and the Breakdown of Cooperation

Consider, for example, the case of a joint venture. Numerous opportunities exist in which it is possible to increase output through cooperation. Unfortunately, however, performance in many such ventures is either impossible or prohibitively costly to monitor.

It follows that a tendency to cheat when there is no possibility of punishment will often eliminate opportunities for mutual gain. Consider a joint venture in which *A* and *B* may either behave honestly or dishonestly. Suppose the payoffs to the four possible combinations of strategies are as given in Table 1, where *H* denotes honest, *D* denotes dishonest, and where $x_4 > x_3 > x_2 > x_1$. Suppose also that the parties interact only once, so that cheating cannot be penalized. These payoffs create a prisoner's dilemma. *D* is the dominant strategy, even though the resultant payoff, x_2, is lower than if each had chosen *H*.[3] Parties *A* and *B* need a commitment device of the sort described by Schelling.

TABLE 1—THE PAYOFFS TO *A* FROM INTERACTION WITH *B*

		B	
		H	*D*
A	*H*	x_3	x_1
	D	x_4	x_2

III. The Conscience as a Commitment Device

Schelling's device worked by altering the relevant material incentives, an approach that will not always be practical. Fortunately, commitment devices can also work in other ways. Consider, for example, a person with the capacity to experience strong feelings of guilt upon breaking a promise—that is, a person with a conscience. This person will often honor his promises even when material incentives favor breaking them. It is precisely this capacity of emotional forces to override rational calculations that makes them candidates for commitment devices.

Of course, merely *having* a conscience does not solve the commitment problem; one's potential trading partners must also know about it. But how can they? Surely it is insufficient for a person merely to *declare* how he or she feels. ("I have a conscience. Trust me.") A strategically important emotion can be communicated credibly only if it is accompanied by a signal that is at least partially insulated from direct control.

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[3] Robert Axelrod (1984), Trivers (1971), and others have demonstrated the tendency for cooperation to emerge in repeated prisoner's dilemmas. My focus here is on whether cooperation can be self-serving when the game is played only once, or, if played more than once, when cheating cannot be detected. For an excellent survey of biological models of cooperation and altruism, see Hirshleifer (1977).

Many observable physical symptoms of emotional arousal satisfy this requirement. Posture, the rate of respiration, the pitch and timbre of the voice, perspiration, facial muscle tone and expression, movement of the eyes, and a host of other readily observable physical symptoms vary systematically with a person's affective condition (Melvin Konner, 1982; Paul Ekman, 1985). In most people, at least some of these symptoms are almost completely insulated from voluntary control (Ekman; Charles Darwin, 1872). If natural selection caused (Darwin), or even permitted, these symptoms to convey such feelings as fear, joy, and grief, there is no reason why it could not have allowed these same symptoms to be related in a characteristic way to feelings of guilt as well.[4]

If there are gains from appearing to be trustworthy, opportunists will inevitably attempt to mimic the symptoms of trustworthiness. Needless to say, character traits would be useless as commitment devices if people who lacked them could *perfectly* mimic their symptoms. But mimicry usually entails costs of its own, and is thus seldom perfect.[5] Granted, lie detection, even the highly technical sort practiced by experienced professionals, is subject to error (David Lykken, 1981). Even so, clues to deceit are reliable in a probabilistic sense and, as will become clear, this is all that is needed for the conscience to serve as a commitment device.

For the sake of discussion, suppose that a mutant did in fact appear with a conscience accompanied by a statistically reliable signal. How might such a mutant have fared? In models of natural selection, a gene for a given trait will increase in frequency relative to its alleles (alternative genes on the same locus) if the trait leads, on average, to a higher material payoff. We must thus compare the mutant's expected payoff to that of persons who lack a conscience. The next section develops a formal model for this purpose.[6]

IV. An Illustrative Model

Consider a population each of whose members bears one of two traits, H or D. Those bearing H are honest, those bearing D are not.[7] To be honest here means to refrain from cheating one's partner in a cooperative venture, even when cheating cannot be punished. To be dishonest means always to cheat under the same circumstances.

Suppose people face one of two options:

1) They may pair with someone in a joint venture whose payoffs are as given above in Table 1. This venture will occur only once and dishonest behavior cannot be punished; or

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[4] Trivers argues that guilt could have evolved within the framework of his reciprocal altruism model as a means of protecting people against the possibility of being caught cheating: "...it is possible that the common psychological assumption that one feels guilt even when one behaves badly in private is based on the fact that many transgressions are *likely* to become public knowledge" (1971, p. 50).

[5] There are abundant cases in nature where mimics coexist with the genuine bearers of a trait for extended periods. For example, there are butterflies, such as the monarch, whose strategy for defending themselves is to have developed a foul taste. This taste itself would be useless unless predators had some way of telling which butterflies to avoid. And so the monarch has developed a conspicuous pattern of wing markings that its predators have learned to interpret for this purpose. The safety afforded by these wing markings has created a profitable opportunity for mutant butterflies who bear similar markings but lack the bad taste that normally accompanies them. Merely by looking like the unpalatable monarchs, viceroy butterflies have escaped predation without having had to expend the bodily resources required to produce the objectionable taste itself. Coexistence of the two types implies that mimicry is either incomplete or else imposes important costs on the viceroy. For an illuminating discussion of the phenomenon of deceptive signaling in animals, see John Krebs and Dawkins (1984).

[6] For a more reader-friendly version of this model, see my forthcoming book (1988, ch. 3).

[7] More precisely, imagine a population with two alleles, a_1 and a_2, at a given genetic locus. Individuals homozygous in a_1 are "honest," those homozygous in a_2 are not. Regardless of what effect honest behavior may have on fitness, each allele in heterozygous individuals will be affected equally by the interactions of these individuals with others. We may thus disregard these individuals when analyzing the competition between a_1 and a_2. In the text, H refers to individuals homozygous in a_1, D to those homozygous in a_2.

2) They may pair with someone in an alternative venture in which behavior can be perfectly monitored. Call this the "work-alone" option, because it does not involve activities that require trust. It too will occur only once, and its payoff is x_2 for each person, irrespective of the combinations of types who interact. Thus, the work-alone option offers the same payoff as when two dishonest persons interact in venture 1.

Many readers will find it more plausible to assume that persons who cheat one another in venture 1 do *worse* than those who participate in ventures that do not require trust (i.e., to assume a payoff larger than x_2 for the work-alone option). But since the goal of this exercise is to see whether honest persons can prosper in the material world, I retain the more conservative assumption that the work-alone option pays only x_2. If honest persons can survive with that payoff, they will certainly do so with an even higher one.

Since working alone pays only x_2, it will never appeal to a dishonest person, who can always do at least as well in venture 1. Its obvious attraction for an honest person is that there is no chance of being cheated; the downside, however, is that its payoff is less than x_3, the gain from successful cooperation in venture 1. (More below on how honest persons choose between the two options.)

In addition to their behavioral differences, let the H's and D's differ with respect to the genes that influence some observable characteristic S, which is also influenced by random environmental forces. More specifically, suppose that the observable characteristic for person i takes the value,

$$S_i = \mu_i + \varepsilon_i,$$

where $\mu_i = \mu_H$ if i is honest, $= \mu_D$ if i is dishonest, $\mu_H > \mu_D$, and ε_i is an independently, identically distributed random variable with zero mean. The μ_i component of S_i is heritable, the ε_i component is not.

Case 1: *A Perfectly Reliable Signal*. If the variance of ε_i were zero, everyone could tell with certainty whether any given person was an H or a D. In that case, H's would pair only with other H's in venture 1, and would receive a payoff of x_3. The D's would be left to pair with one another, for which they would receive only x_2. Thus, if the signal that accompanied each trait were *perfectly* reliable, the H's would soon drive the D's to extinction.

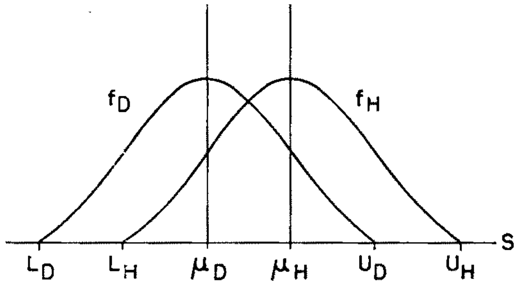

FIGURE 1. PROBABILITY DENSITIES FOR AN OBSERVABLE SIGNAL OF TRUSTWORTHINESS

Case 2: *Imperfect Signals*. By contrast, when ε_i has sufficiently large variance, S_i provides merely a measure of the probability that i is trustworthy. As an illustration of this more interesting case, suppose people draw their S values independently from the probability densities f_D and f_H shown in Figure 1.[8]

In terms of the earlier discussion, the relative position of the two densities reflects imperfect mimicry by the D's of the trait used by the H's to identify themselves as trustworthy. For the densities shown, whose ranges do not overlap completely, we are sure that individuals with $S > U_D$ are honest, and that those with $S < L_H$ are dishonest. Individuals whose S values lie in the region

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[8] Each density shown in Figure 1 is derived from the standard normal density, $f(S)$, in the following manner: First the tails are truncated at two standard deviations from the mean by subtracting $f(2)$ from every point. Let I denote the area under the resulting functions. The functions are then normalized by dividing each by I. Letting 2 and 3 be the respective means, we thus have

$$f_H = (1/\sqrt{2}\pi)(1/I)\left[\exp\left(-(x-3)^2/2\right)-\exp(-2)\right],$$

and

$$f_D = (1/\sqrt{2}\pi)(1/I)\left[\exp\left(-(x-2)^2/2\right)-\exp(-2)\right].$$

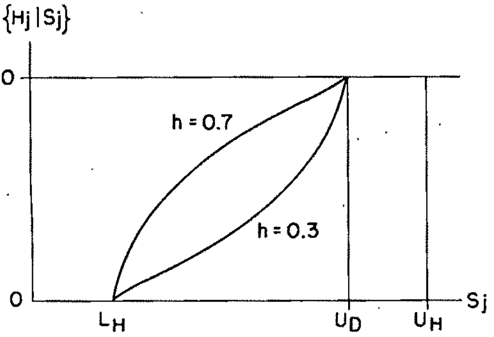

FIGURE 2. THE PROBABILITY OF BEING HONEST, CONDITIONAL ON S

where the two densities overlap may be either H or D. If h denotes the proportion of the population bearing H, the probability that an individual j with $S=S_j$ is honest is then given by

$$(1)\quad \Pr\{H_j|S_j\}=\frac{hf_H(S_j)}{hf_H(S_j)+(1-h)f_D(S_j)}.$$

For the two densities shown in Figure 1, $\Pr\{H_j|S_j\}$ is plotted in Figure 2 for two different values of h.

A. *The Threshold Signal Value*

Given the payoffs in Table 1, both H's and D's will clearly do best by interacting (if they choose venture 1) with other individuals whom they believe to be H's. Consider the problem confronting individual i, who is H and must decide whether to interact with individual j, who has $S=S_j$. Let $E(X_{ij}|S_j)$ denote the expected payoff to i from pairing with this individual. If the alternative is the work-alone option (which has payoff x_2), the condition that makes it worth i's while to work with j is given by

$$(2)\quad E(X_{ij}|S_j)=x_3\Pr\{H_j|S_j\}+x_1\left[1-\Pr\{H_j|S_j\}\right]\geq x_2.$$

Call S^* the value of S_j that satisfies $E(X_{ij}|S_j)=x_2$. The value S^* is the smallest value of S_j for which it would be as attractive for i to work with j as to work alone. Using the densities from Figure 1, this threshold signal value is shown in Figure 3 for the particular case of $h=1/2$ and $x_2=(x_1+x_3)/2$.

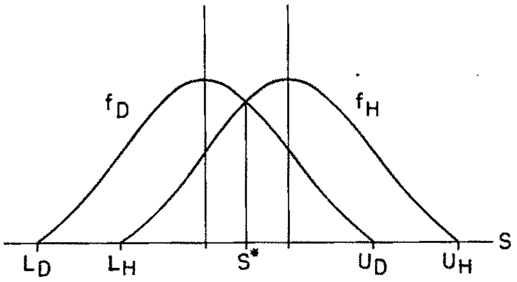

FIGURE 3. THE THRESHOLD SIGNAL VALUE WHEN $h=1/2$ AND $x_2=(x_1+x_3)/2$

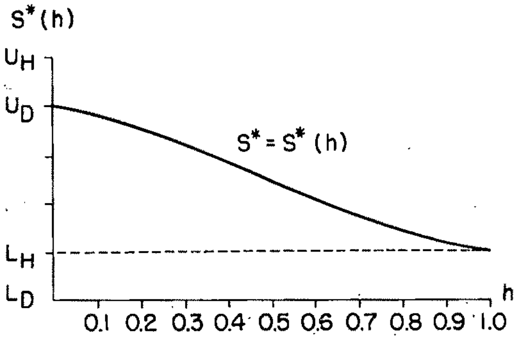

FIGURE 4. THE THRESHOLD SIGNAL VALUE AS A FUNCTION OF h

Using equations (1) and (2), it can easily be shown that S^* is a decreasing function of h: As the proportion of the population that is honest grows, it becomes ever more probable that an individual whose S lies between L_H and U_D is honest, and ever more likely, therefore, that the expected payoff from interacting with that individual will exceed x_2. It is also easy to show that S^* decreases with increases in (x_3-x_2) and increases with increases in (x_2-x_1). The greater the payoff to successful cooperation, and the smaller the penalty for being cheated, the lower will be the threshold signal for cooperation. For the particular case of $(x_4-x_3)=(x_3-x_2)$

$=(x_2-x_1)=x_1$, the schedule $S^*(h)$ that corresponds to the densities from Figure 1 is plotted in Figure 4.

B. *The Sorting Process Whereby Interacting Pairs Form*

It is not strictly correct to say that an individual who is honest faces only the alternatives of working alone or working with someone with $S=S_j$. There are other members of the population besides j; and, of those with $S>S^*$, i will want to choose a partner with the highest possible S value. The problem is that *everyone* prefers to interact with such a partner, and there are only so many of them to go around.

The bearer of a high S value, whether he is honest or not, owns a valuable asset. The natural way to exploit it will be to use it to attract a partner whose own S value is also high. Without going into technical details, it can be seen that the outcome will be for the two with the highest S values to pair up, then the next two, and so on, until all those with $S \geq S^*$ are paired. Needless to say, the expected total payoff to any pair increases with the S values of its members. If the population is sufficiently dense, as assumed here, the S values within each pair will be virtually the same.[9]

C. *The Expected Payoff Functions*

With the foregoing discussion in mind, let us now investigate what happens if a mutation provides a small toehold for the H's in a population that initially consisted entirely of D's. Will the H's make headway or be driven to extinction? To answer this question, we must compare the average payoffs for H's and D's, denoted $E(X|H)$ and $E(X|D)$, respectively, when h is near zero. To calculate $E(X|H)$, note from equations (1) and (2) that $S^*(h)$ approaches U_D as h approaches zero (see Figure 4). As the share of the population that is honest approaches zero, the rational strategy for an honest person, in the limit, is to interact only with people whose S values lie to the right of U_D —that is, with people who are certain to be honest. As h approaches zero, the expected payoff to the H's thus approaches the limiting value

$$(3)\quad \lim_{h\to 0} E(X|H)=x_3\int_{U_D}^{U_H} f_H(S)\,dS + x_2\int_{L_H}^{U_D} f_H(S)\,dS.$$

The second term on the right-hand side of equation (3) reflects the fact that an H with $S<U_D$ does best by not interacting—that is, by choosing the work-alone option. (He would be delighted to interact with an H with $S>U_D$, but that person would not agree to interact with *him*.)

The corresponding limiting expected payoff for the D's is simply x_2. So when the proportion of the population that is honest is very small, we have $E(X|H)>E(X|D)$, which means h will grow.

What if we had started at the opposite extreme—in a population in which the proportion of D's was near zero? In the limit, the decision rule for the H's would be "interact with anyone whose S value exceeds L_H." As h approaches one, the expected payoff for the D's would thus approach

$$(4)\quad \lim_{h\to 1} E(X|D)=x_2\int_{L_D}^{L_H} f_D(S)\,dS + x_4\int_{L_H}^{U_D} f_D(S)\,dS.$$

The corresponding expected payoff for H will approach x_3 as h approaches one. Comparing the right-hand side of equation (4) with x_3, we see that the former will be larger for sufficiently large values of x_2 or x_4, or for sufficiently extensive overlap of the ranges of f_H and f_D. Assuming that $E(X|D)$ does exceed x_3 for values of h close to one, h will then begin to fall.

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[9] In less dense populations, S values could differ within pairs, suggesting that the member in any pair with the lower S value might have to pay his partner in order to induce him to participate. Any such transfer payments will obviously not affect the average payoff to members of a cooperating pair. Even if the assumption of dense populations did not hold, therefore, complications of this sort would create no difficulties: We are concerned only with average payoffs to the two genotypes here.

To investigate the behavior of this population as it evolves away from either of the two pure states, we need general expressions for $E(X|H)$ and $E(X|D)$.

The first step in finding $E(X|H)$ is to use equations (1) and (2) to find S^* as a function of h (as was done to generate Figure 4). Given $S^*(h)$, we then calculate P_H and P_D, the respective shares of the honest and dishonest populations with $S > S^*$:

$$P_H = \int_{S^*}^{U_H} f_H(S)\,dS; \tag{5}$$

$$P_D = \int_{S^*}^{U_D} f_D(S)\,dS. \tag{6}$$

Given equations (5) and (6), $E(X|H)$ can be computed as a weighted sum of the payoffs from (1) working alone, (2) interacting with someone who is honest, and (3) interacting with someone who is dishonest. Noting that $(1-P_H)$ is the probability that an honest individual works alone, and using $\lambda = hP_H/(hP_H+(1-h)P_D)$ to denote the share of the population with $S > S^*$ that is honest, we have

$$E(X|H) = (1-P_H)x_2 + P_H[\lambda x_3 + (1-\lambda)x_1]. \tag{7}$$

In like fashion, $E(X|D)$ can be calculated as a weighted sum of the payoffs to working alone, interacting with someone who is honest, and interacting with someone who is dishonest:

$$E(X|D) = (1-P_D)x_2 + P_D[\lambda x_4 + (1-\lambda)x_2]. \tag{8}$$

For my illustrative densities from Figure 1, $E(X|H)$ and $E(X|D)$ are plotted as functions of h in Figure 5. Both $E(X|H)$ and $E(X|D)$ will in general be increasing functions of h. For the particular densities from Figure 1, the two curves cross exactly once.

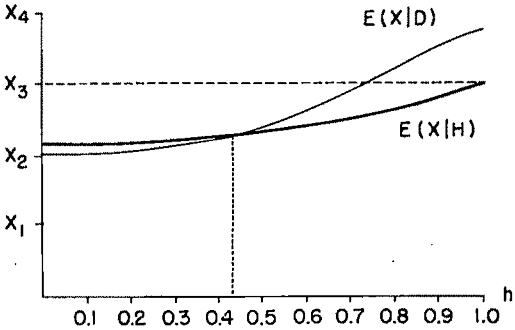

FIGURE 5. EXPECTED PAYOFFS FOR H AND D

The equilibrium value of h is the one for which $E(X|D)$ and $E(X|H)$ intersect. For an equilibrium to be stable, $E(X|H)$ must cross $E(X|D)$ from above. Since $E(X|H)$ will always be greater than $E(X|D)$ at $h=0$, the intersection of the two curves (assuming they intersect at all) will thus correspond to a stable equilibrium value of h, like the one depicted in Figure 5.

Case 3: *Costs of Scrutiny*. The model can be made more complete, and the existence of an equilibrium assured, if we add the assumption that resources must be expended in order to inspect the S values of others. Let us assume specifically that these values can be observed only by people who have borne the cost C of becoming sensitized.

First consider the limiting case of a population in which h approaches zero. A necessary condition for any interaction to occur at all is that $x_3 - C > x_2$. Assuming that condition is met, only those honest persons whose own S values exceed U_D will find it worthwhile to spend C to become sensitized. H's with $S < U_D$ will work alone (because there is no prospect of their finding an H with whom to interact). Thus the latter H's will receive payoff x_2, the same as $E(X|D)$ when h is zero. No D's will pay to become sensitized because they too have no chance of finding an unclaimed H, so the expected payoff for D's here is x_2. For very small values of h, then, some H's will fare the same as the D's, others better. Thus, as before, h will grow when it starts near zero.

For values of h near one, no one will find it worthwhile to pay C to become sensitized. With everyone's S thus effectively out of sight, even D's with $S < L_H$ will find honest partners with whom to interact. $E(X|D)$ will thus approach x_4 as h approaches one. So, unlike the case where S could be observed for free, it is no longer possible for the H's to take over the entire population, regardless of how close x_3 may be to x_4.

A key step in understanding the nature of the equilibrium that results when h starts near zero is to recognize that it will always be most strongly in the interests of those with the highest S values to bear the expense of becoming sensitized. To see this, recall from the earlier discussion that the bearer of a high S value owns a valuable asset he can use to attract a partner whose own S value is also high. It is in the interests of bearers of high S values to interact with others who are H. And, having become sensitized, they are then able to identify others with high S values (the people who are most likely to be H) and interact with them. Persons with relatively low S values, who can only hope to claim a partner whose own S value is also low, therefore face a lower expected payoff to the investment of becoming sensitized.

Knowing this enables one to conclude that the equilibrium h must be sufficiently low that it pays at least *some* of the population to become sensitized. This can be shown by noting that, if the contrary is assumed, we get a contradiction. That is, suppose it is assumed that, in equilibrium, the expected payoff to the person with the highest S value is higher if he does *not* become sensitized. Recall that his expected payoff if he had become sensitized, call it $E(X,C)$, would be

$$E(X,C) = x_3 - C, \tag{9}$$

which is already assumed to be greater than x_2. We know that if it does not pay the H with the highest S value to become sensitized, it does not pay any other H to do so either. And when nobody pays C, the odds of H's with low S values pairing successfully are the same as for those with high S values. *All* of the H's must thus expect a payoff higher than x_2. This means it pays all of the H's to interact, and all will expect the same payoff, call it $E(X,0)$:

$$E(X,0) = hx_3 + (1-h)x_1. \tag{10}$$

But the expected payoff for each D when everyone interacts is $hx_4 + (1-h)x_2$, which is clearly larger than $E(X,0)$.

Thus, if it is assumed that the equilibrium h is so large that it does not pay even those with the highest S values to become sensitized, we are forced to conclude that the expected payoff to D exceeds the expected payoff to H at that value of h. This means that it could not have been an equilibrium value after all. The equilibrium h must therefore be small enough that it is in the interests of at least those with the highest S values to become sensitized.

To gain further insight into the nature of the equilibrium here, let us again consider a population in which h started near zero and has grown to some positive value, say, h_0. At h_0, who will interact with whom, and in whose interests will it be to become sensitized? We can show it will pay those persons with S greater than some threshold value, $S^*(h_0) < U_D$, to become sensitized and interact with others with like S values. And we can also show that it may or may not pay H's with $S < S^*(h_0)$—that is, those who do not find it profitable to become sensitized—to interact at random with other members of the truncated population (people with $S < S^*(h_0)$).

The threshold value $S^*(h_0)$ is determined as follows. Again making use of the notation defined in equations (5) and (6), note that

$$1 - P_H = \int_{L_H}^{S^*(h_0)} f_H(S)\,dS; \tag{11}$$

$$1 - P_D = \int_{L_D}^{S^*(h_0)} f_D(S)\,dS, \tag{12}$$

are the respective shares of the honest and dishonest populations for which $S < S^*(h_0)$. Let $\delta = (1 - P_H)h_0/[(1 - P_H)h_0 + (1 -$

$P_D)(1-h_0)]$ denote the share of this truncated population that is honest. $S^*(h_0)$ is the value of S that solves the following equation:

$$\text{(13)}\quad \Pr\{H|S^*(h_0)\}x_3 + (1-\Pr\{H|S^*(h_0)\})x_1 - C = \max\{x_2, \delta x_3 + (1-\delta)x_1\}.$$

The left-hand side of equation (13) is the expected payoff to an H with $S = S^*(h_0)$ who pays C to become sensitized and then interacts with someone with the same S value (see equation (1)). The second expression within the brackets on the right-hand side of equation (13) is the expected payoff to that same H (or, for that matter, to any other H) if he does not become sensitized and interacts with a randomly chosen person from the truncated population (again, those D's and H's with $S < S^*(h_0)$). If x_2 is larger than that figure, as it is likely to be for small values of h_0, all H's with $S < S^*(h_0)$ will work alone. Otherwise all H's will interact —those with $S > S^*(h_0)$ interacting with others with $S > S^*(h_0)$, those with $S < S^*(h_0)$ interacting with randomly chosen members of the remaining population. Depending on the particular densities used for f_H and f_D, either outcome is possible.

Once $S^*(h_0)$ is determined, it is straightforward, albeit tedious, to calculate $E(X|H)$ and $E(X|D)$. Using the rule that h grows when $E(X|H) > E(X|D)$, the equilibrium value of h is the one for which the two expected payoff curves cross. If we again employ the densities from Figure 1, and use $C = x_1/4$, the expected payoff curves and the equilibrium h are as depicted in Figure 6.

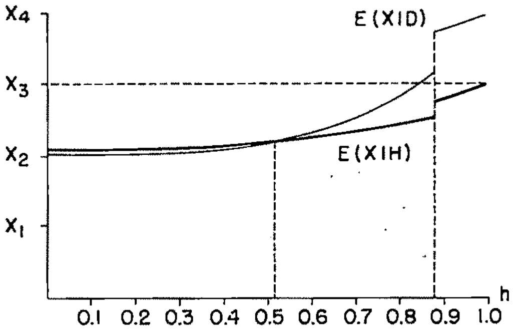

FIGURE 6. EQUILIBRIUM WHEN THERE ARE COSTS OF SCRUTINY

Two features of Figure 6 merit discussion. Note first the discontinuities in the two payoff curves. To see why these occur, recall that it would not be in the interests of anyone to pay the costs of scrutiny if the population initially consisted almost entirely of H's. All of the H's, even those with the highest S values, expect a higher payoff interacting at random with people from the population at large. For values of h close to one, the expected payoff to the D's exceeds that to the H's, which, as noted earlier, causes h to fall. Once h reaches the value for which

$$\text{(14)}\quad E(X,0) = hx_3 + (1-h)x_1 = x_3 - C,$$

however, it abruptly becomes in the interests of those H's with $S > U_D$ to become sensitized, thereby to interact selectively with one another, rather than with individuals chosen at random. At the value of h that solves equation (14), the H's who incur the costs of becoming sensitized are exactly compensated for those costs by the higher gains from selective interaction. But their sudden unavailability for interaction with the remainder of the population produces an abrupt decline in everyone else's expected payoff. This decline is reflected in the discontinuities in the expected payoff curves in Figure 6.

Note also that the equilibrium value of h in Figure 6 is larger than the corresponding value in Figure 5. Although the costs of scrutiny reduce the expected payoffs to both groups in absolute terms, the reduction is more pronounced for the D's. This happens because the costs of scrutiny cause the schedule $S^*(h)$ to shift to the right. And given that f_D lies to the left of f_H, any such shift in $S^*(h)$ will cause a proportionally greater reduction to the D's payoff than to the H's. It is perhaps fitting that, in their struggle with the D's, the H's are abetted by the fact that it requires some effort to observe the symptoms of trustworthiness.

This model treats people as if they were either honest or dishonest. In reality, however, honesty is not a binary trait. A more fully articulated theory would likely predict the coexistence of honest and dishonest impulses within each individual. The resulting environment would be one in which people search hopefully for others they can trust, while remaining ever vigilant against the possibility of being cheated—in short, an environment very much like the one that confronts us.

This outcome is at once in harmony with the economists' view that people pursue self-interest and with their critics' view that people often transcend selfish tendencies. The clear irony is that the gains from cooperation cannot be fully exploited here unless one adopts at the outset what everyone would call a genuinely unselfish point of view.[10]

The model thus suggests that people will often refrain from cheating not because they fear being caught, but because cheating simply makes them feel bad. This view is consistent with an extensive body of empirical evidence assembled by Jerome Kagan; as he summarizes his interpretation of that evidence:

> Construction of a persuasive rational basis for behaving morally has been the problem on which most moral philosophers have stubbed their toes. I believe they will continue to do so until they recognize what Chinese philosophers have known for a long time: namely, feeling, not logic, sustains the superego. [1984, p. *xiv*]

Feelings may indeed sustain the superego. But in the model discussed here, it is logic that ultimately sustains those feelings.

V. Other Aspects of the Utility Function

A. *Concerns About Fairness*

Like feelings of guilt, concerns about fairness may also play a useful role in the utility function.[11] People often walk away from profitable opportunities when they perceive the terms of a transaction to be "unfair." To make sense of such behavior in the framework of conventional economic models, we must imagine that they are investing in the development of reputations for being tough bargainers. That may sometimes be so. But much of the time people walk away just because they would feel even worse if they did not. And even the strategy of developing a tough reputation would be much easier to execute if people actually *disliked* participating in profitable, but one-sided, transactions. People who felt that way would not be lying when they threatened to abandon negotiations. Their preference structure would serve, in effect, as a commitment device, one that makes it in their interests to reject an unfair bargain.

B. *Anger and Vengeance*

Anger, too, often motivates behavior that does not appear self-serving. Customers who are wronged by a supplier may give up several days of their time and suffer much inconvenience in the process of tracking down a $50 refund. People often behave this way even when they are visiting distant locations, where there is no reasonable prospect that their behavior will help them develop reputations for toughness. The proximate cause is that behaving any other way would leave them feeling angry and frustrated.

Viewing tastes as commitment devices suggests a clear rationale for such behavior. If a capacity for anger is observable in a person, suppliers will know he is commited to seek redress, even if it does not "pay" him to do so. This, in turn, makes it rational for the buyer to trust the supplier, even when some dimensions of product quality cannot be observed before purchase. The buyer's capacity for anger thus benefits both himself *and* the supplier. For, as George Akerlof's lemons model demonstrates (1970), the alternative here would be for no transaction to take place at all.

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[10] This is not to say, of course, that we never encounter the more narrowly selfish utility function generally assumed by economists. People of the sort who inhabit economic models surely do exist; but most of us (economists included!) make every effort to steer clear of them.

[11] See my book (1985b, chs. 2,5,6).

VI. Concluding Remarks

Tastes are important. Seemingly minor alterations in the utility function can produce large changes in the conclusions that emerge from economic models. As I have emphasized elsewhere (1984a, b; 1985a, b), the presence of concerns about relative position will affect the wages people earn and also how they spend them. Similarly, the presence of a capacity for anger makes it easier to understand a variety of behaviors that are difficult to rationalize by reference to utility functions of the usual sort.[12] Finally, conscience and other moral sentiments also play a powerful role in the choices people make.

Modern economists regard behaviors arising from these emotions as lying outside the scope of our standard model. But it is neither necessary nor productive to view them this way. Our utility-maximization framework has proven its usefulness for understanding and predicing human behavior. With more careful attention to the specification of the utility function, the territory to which this model applies can be greatly expanded.

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[12] Hirshleifer (1978b) makes a similar point.

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The Capital-Energy Complementarity Debate Revisited

By John L. Solow*

This paper argues that the empirical disagreement as to whether capital and energy are complements or substitutes is not likely to be reconciled with aggregate data. It demonstrates that price-induced changes in the composition of output can cause either outcome in the aggregate, even if no technical substitution is possible. Substitution by consumers and changes in the relative incomes of consumers and foreigners are identified as key factors in determining which outcome arises.

Perhaps the most important practical use of empirical work is estimation of the parameter values that are necessary for analytical apparatus to be of use in evaluating the impacts of events or policies. So it is particularly disturbing when conflicting estimates of an important set of parameters is the result. Such has been the case in the estimation of elasticities of substitution between energy and other inputs, which are crucial for understanding the macroeconomic impacts of energy price shocks,[1] and for evaluating energy policy. The immediacy of these problems rises and falls with the price of oil, but the question remains important.

A substantial number of empirical analyses of this issue now exist (see Ernst Berndt and David Wood, 1979). These typically involve the estimation of a translog cost function using data from the manufacturing sector. The results are notably contradictory, with some finding that capital and energy are complements (for example, Edward Hudson and Dale Jorgenson, 1974; Berndt and Wood, 1975) and others finding that capital and energy are substitutes (for example, James Griffin and Paul Gregory, 1976; Robert Pindyck, 1979).

Several attempts have been made to explain or reconcile these differences. One is the claim by Griffin and Gregory, Pindyck, and Griffin (1981a,b) that estimates based on international cross-sectional data, which typically show capital and energy to be substitutes, measure the long-run relationship between inputs. Studies that make use of time-series data, they argue, capture short-run factor relationships because adjustments to factor prices take time; these studies usually find that capital and energy are complements. The idea that capital and energy are complements in the short run but substitutes in the long run is supported by some estimates of explicitly dynamic models (see, for example, Pindyck and Julio Rotemberg, 1983), but others conclude that short-run capital-energy complementarity becomes stronger in the long run (see Catherine Morrison and Berndt, 1981). Building on the theoretical contributions of David Stapleton (1981), Nalin Kulatilaka (1985) has tested the Berndt-Wood data to see if they provide long-run elasticity estimates. While he finds that they do not, his results also indicate that the long-run elasticities exhibit stronger, not weaker, capital-energy complementarity.

Another avenue for reconciliation of these diverse results is offered by Berndt and Wood (1979). They note that the studies that find capital-energy substitutability generally involve only three inputs (capital, labor, and energy), while those that find complementarity usually include a fourth (materials). They demonstrate that these findings are not theoretically inconsistent, and argue that the finding of substitutability is due to the omission of the fourth factor. While this argument is theoretically compelling, the existence of some four-input studies that find capital-energy substitutability (for example, Michelle Turnovsky et al., 1982) means that this cannot be all of the story.

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[1] See, for instance, Stanley Fischer (1983) and the references therein.

Other authors suggest the need to disaggregate capital inputs into working capital and physical capital, but incorporating such a distinction has failed to resolve the empirical debate. Barry Field and Charles Grebenstein (1980) find that energy is generally a complement to physical capital and a substitute for working capital, but Raymond Kopp and V. Kerry Smith (1978) find precisely the reverse. Griffin (1981b) discusses some of the weaknesses of these studies, and points out further that there is as yet no well-developed theoretical basis for incorporating working capital into cost function estimation.

Finally, Wood and Robert Hirsch (1981) argue that many of the differences among the Berndt-Wood, Griffin-Gregory, and Pindyck results are due to various measurement issues, particularly with regard to the inclusion of taxes in the capital service price. There remains some disagreement about exactly what was included by whom (see the exchange in Griffin, 1981b, and Berndt-Wood, 1981), but in any case, this explanation does not reconcile differences among the large number of other estimates which are also part of the controversy.

The purpose of this paper is to suggest that reconciliation of these estimates may not be possible, because these studies apply a partial-equilibrium model to data which are sufficiently aggregated that general equilibrium responses can occur. I focus on changes in the composition of output that result from an energy price increase. If manufacturing output consists of goods that have different energy intensities, then as energy prices vary, compositional changes will take place concurrently with the factor substitution that occurs within the production of each good. Since both of these effects are systematically related to changes in energy prices, we cannot expect estimations using aggregate data to sort them out. Indeed, I show that, even if no technical substitution is possible, aggregate data can exhibit what appears to be substitutability or complementarity among inputs arising from compositional changes alone.

The model that will be presented is not intended to be either quantitatively or descriptively accurate. It is an extremely stylized model intended to isolate some of the effects of energy price shocks, and to address the implication of those effects for the capital-energy complementarity debate. I will consider an economy that faces an exogenous price of energy determined in the world market, and examine how its general equilibrium responds to changes in that price. To suppress the impact of factor substitution and isolate the compositional effects, I will assume that production is governed by fixed factor ratios. On the other hand, I will differentiate between energy-intensive and nonenergy-intensive goods, and assume that consumer demands are sensitive to the relative prices of these goods. Variations in the price of energy, by affecting these relative prices, will induce changes in the composition of output that result in changes in the derived demand for factors. The model demonstrates that these changes can produce what appears to be technical substitution in the aggregate when in fact none need be occurring.

This is more than simply a restatement of the proposition that, although production is governed by fixed factor ratios at the sectoral level, an economy can exhibit smooth factor substitution in the aggregate due to compositional changes (see Hendrik Houthakker, 1955). The importance of this model comes from the isolation of a particular set of general equilibrium effects that lead to those compositional changes which are systematically related to the price of energy. For example, Graciela Chichilnisky and Geoffrey Heal (1983), in a model similar in many respects to the one to be considered here, also focus on the impacts of energy price shocks working through changes in the composition of output, but through an entirely different general equilibrium mechanism. Chichilnisky and Heal also consider an economy that faces an exogenous price of energy and that produces two goods according to fixed factor ratios. However, they distinguish between suppliers of capital services and labor in the economy, and assume that capital-owners and foreigners consume only one good, while laborers consume only the other. Since each consuming group consumes only one good, changes in relative prices do not induce substitution in consumption in the usual sense. Instead, relative price changes lead to changes in the functional distribution of real income which, given the particular assumptions about demands, cause changes in the composition of output and the derived demand for factors. This alternative mechanism for driving changes in the composition of output may also matter; the question is whether either or both of these results are empirically important.

If the effects described here are to be empirically relevant, factor intensities have to differ sufficiently across goods; if they do not, compositional changes resulting from changing relative output prices are likely to be small (although they might result from other things, such as differences in income elasticities of demand). Several studies have shown that energy intensities do vary widely (see Clark Bullard and Robert Herendeen, 1975; Bullard et al., 1978; Bruce Hannon, 1974; and my paper with Deborah Johnson, 1984). And while these differences are not sufficient to ensure large compositional effects, there is some evidence to suggest that those responses do matter. Robert Marlay (1984), examining changes in U.S. energy demand from 1973 to 1982, found "unprecedented changes in the composition of output away from industries that consume large amounts of energy" (p. 1277), noting further that, "...no change of like magnitude or duration was observed before 1970" (p. 1279). Marlay's analysis is not capable of discerning what part of those changes are a response to higher energy prices, but it seems highly unlikely that they have little effect. A more troublesome aspect of Marlay's attribution of almost one-third of the reduction in energy use over this period to compositional changes is his use of engineering, not economic, measurement concepts.

Although it is not the focus of their work, Antonio Borges and Lawrence Goulder (1984) also provide evidence that compositional changes are important. They use a disaggregated general equilibrium model to examine the long-run response of the U.S. economy to higher energy prices; the model incorporates 19 nonenergy production sectors. Under the assumption that each sector's technology can be represented by a Cobb-Douglas production function, they find average annual sectoral growth rates that are, with few exceptions, highest for the least-energy-intensive sectors and lowest for the most-energy-intensive sectors. Although the growth rate differences are small in absolute magnitude, the model's time horizon is twenty-eight years, so that quite large compositional changes are occurring. Moreover, Cobb-Douglas production is almost certainly an overstatement of factor substitutability, so that these results represent a lower bound on the size of compositional shifts. The Borges and Goulder results have the advantage that they represent a controlled simulation; the only thing driving the model is energy price increases. Their major drawback is that the model is not empirically estimated; instead, functional forms are chosen and benchmarked to a consistent data set. Some parameters are taken from empirical estimates, but many are imposed in the specification of the functional forms.

Hudson and Jorgenson have developed a 36-sector general equilibrium model that is based on econometrically estimated parameters, which might provide empirical evidence as to the importance of compositional responses to energy price increases. However, to date that model has not been applied to the evaluation of higher energy prices or energy taxes.[2] The earlier, 9-sector version of this model has been applied to these issues (see, for example, Jorgenson, 1982). However, the degree of aggregation in that model (5 of the sectors produce energy goods; the others are Agriculture, Nonfuel Mining and Construction; Manufacturing; Transportation; and Communications, Trade

[2] Personal communication with Dale Jorgenson.

and Services) makes it subject to precisely the question that is raised here, namely, what part of the factor substitution in the Manufacturing sector is in fact the result of changes in the composition of that sector's output.

In the next section of the paper, I present the general equilibrium model. Section II examines some of the possible responses to energy price increases this economy can exhibit, and Section III concludes the paper.

I. The Model

I consider an economy that produces two goods, X and Y. The X goods are assumed to be energy intensive, and Y goods are assumed to be nonenergy intensive, in a sense that will be made clear shortly. Production requires three inputs: capital, labor, and energy. All capital and labor inputs are supplied by consumers in the domestic economy, who spend all of their income on the two goods. There is no saving. All energy inputs are imported from the rest of the world[3] at an exogenous price, and the rest of the world purchases exports of the two goods with its revenue so that trade is balanced. All markets are assumed to be perfectly competitive.

Factor supplies are assumed to be linear[4] functions of their real returns;

$$K=\delta_K P_K / I(P_X, P_Y) \tag{1}$$

$$L=\delta_L P_L / I(P_X, P_Y) \tag{2}$$

where K and L denote total capital and labor supply, respectively, P_K, P_L, P_X, and P_Y denote the prices of capital services, labor services, X goods and Y goods, respectively, and $I(P_X, P_Y)$ is a monotonically increasing index of consumption goods prices. δ_K and δ_L are positive parameters.

Consumers are assumed to have Cobb-Douglas utility, so that consumer demands can be expressed as[5]

$$X_C=\varepsilon_C(P_K K+P_L L)/P_X \tag{3}$$

$$Y_C=(1-\varepsilon_C)(P_K K+P_L L)/P_Y \tag{4}$$

where X_C and Y_C denote domestic consumption of X goods and Y goods, and ε_C and $(1-\varepsilon_C)$ are the parameters of the utility function.

The rest of the world is also assumed to have Cobb-Douglas utility, so that the demands for exports are similar to domestic consumer demands;

$$X_R=\varepsilon_R P_E E/P_X \tag{5}$$

$$Y_R=(1-\varepsilon_R) P_E E/P_Y \tag{6}$$

where X_R and Y_R denote exports of X goods and Y goods, P_E denotes the exogenous price of energy, and E denotes total imports of energy by the domestic economy.

I assume that production of each good is characterized by a Leontief production function. That is,

$$X_C+X_R = \min(K_X/\alpha_X, L_X/\beta_X, E_X/\gamma_X) \tag{7}$$

where K_X, L_X and E_X denote inputs of capital, labor, and energy, respectively, into production of good X. α_X, β_X and γ_X are positive parameters. Similarly,

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[3] The assumption that all energy is imported is made to focus the analysis on the impact of changes in the composition of consumption goods output on factor use. If energy is also produced domestically, higher energy prices will lead to expansion of the domestic energy sector, thus driving up the price of factors used relatively intensively in that sector. For a further discussion of issues related to the omission of domestic energy production from energy-economy models, see David Burgess (1979) and Stephen Peck and myself (1982).

[4] The assumption of linearity is only made to keep the nature of the model simple (i.e., quadratic at worst). Any upward-sloping supply functions would provide qualitatively similar results. Of course, if factor supplies are perfectly inelastic, energy prices will have no effect on the aggregate factor ratio.

[5] All consumers are assumed to have identical utility functions, and since the Cobb-Douglas utility function is homothetic, changes in the distribution of income will not alter relative demands for goods. Alternatively, it could be assumed that all consumers have equal endowments of both capital and labor, or that there is only one consumer.

$$(8)\quad Y_C + Y_R = \min(K_Y/\alpha_Y, L_Y/\beta_Y, E_Y/\gamma_Y)$$

where for now all we require of the parameters of the Y goods production function is that they not all be a common multiple of their counterparts in the X goods production function. By definition, $K_X + K_Y = K$, $L_X + L_Y = L$, and $E_X + E_Y = E$.

Efficient use of factors in production of the two goods implies that demand for capital, labor, and energy are given by

$$(9)\quad K = \alpha_X(X_C + X_R) + \alpha_Y(Y_C + Y_R)$$

$$(10)\quad L = \beta_X(X_C + X_R) + \beta_Y(Y_C + Y_R)$$

$$(11)\quad E = \gamma_X(X_C + X_R) + \gamma_Y(Y_C + Y_R)$$

and output prices are given by

$$(12)\quad P_X = \alpha_X P_K + \beta_X P_L + \gamma_X P_E$$

$$(13)\quad P_Y = \alpha_Y P_K + \beta_Y P_L + \gamma_Y P_E.$$

Since only relative prices matter, we are allowed the choice of one commodity to serve as numeraire. We will let Y goods serve this purpose, so that

$$(14)\quad P_Y \equiv 1.$$

Equations (1)–(6) and (9)–(14) constitute a system of twelve equations in the eleven endogenous variables and one exogenous variable of the model. The endogenous variables are the supplies of capital and labor, consumptions of X goods and Y goods by domestic consumers and the rest of the world, the level of energy imports, and the prices of capital services, labor services, X goods, and Y goods. The price of energy is the exogenous variable. If we know that four of the five markets in the model are in equilibrium, then the fifth must be also, and thus the model is not overdetermined. I wish to examine how the equilibrium of this system changes as the price of energy changes. In particular, I wish to examine the responses of equilibrium aggregate factor ratios to a changing energy price.

Substitution of the equations of the model to eliminate variables enables us to reduce the model to one equation giving P_X as an implicit function of P_E (see the Appendix for details):

$$(15)\quad A_1 P_X^2 + A_2 P_X + A_3 P_X P_E + A_4 P_E + A_5 P_E^2 + A_6 = 0,$$

where
$$A_1 = (1 - \varepsilon_C)(\beta_Y^2 \delta_K + \alpha_Y^2 \delta_L)$$
$$A_2 = (1 - 2\varepsilon_C)(\beta_X \beta_Y \delta_K + \alpha_X \alpha_Y \delta_L)(-1)$$
$$A_3 = (1 + \varepsilon_R - 2\varepsilon_C)(\beta_Y F \delta_K + \alpha_Y G \delta_L)$$
$$A_4 = (\varepsilon_R - 2\varepsilon_C)(\beta_X F \delta_K + \alpha_X G \delta_L)(-1)$$
$$A_5 = (\varepsilon_R - \varepsilon_C)(F^2 \delta_K + G^2 \delta_L)$$
$$A_6 = \varepsilon_C(\beta_X^2 \delta_K + \alpha_X^2 \delta_L)(-1)$$

$F = \beta_X \gamma_Y - \beta_Y \gamma_X$ and $G = \alpha_X \gamma_Y - \alpha_Y \gamma_X$. Since X goods are assumed to be energy intensive, I assume that the energy-capital and energy-labor ratios in the X sector exceed those in the Y sector, and that F and G are thus negative.[6] I also assume that the two goods have different capital-labor ratios; otherwise, the capital-labor ratio in production would fix the equilibrium factor supply ratio, given the Leontief production functions. It is also useful at this point to choose units of measurement of goods and factors so that, without loss of generality, α_X, β_X, γ_X, and γ_Y equal unity. My assumptions about factor intensities now translate into the assumptions that α_Y and β_Y are not less than unity, and not equal to each other. Note that even with this normalization, A_1 through A_6 still depend on six independent parameters: α_Y, β_Y, δ_K, δ_L, ε_C, and ε_R. Given values for these parameters and for P_E, I can calculate P_X from equation (15), and then generate the remaining endogenous variables from the equations in the Appendix. This enables us to see how the general equilibrium adjusts to changes in the price of energy. While analytic results are difficult to derive, it is straightforward to solve the model numerically; equation (15) can be written as a quadratic function of P_X where the coefficients involve P_E and parameters, and the quadratic formula can be applied.

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[6] There is an interesting possibility here that the X goods have a higher energy-labor ratio but a lower energy-capital ratio than the Y goods, or vice versa. In this case, it is not clear what it means to say that the X goods are energy intensive. There is some empirical evidence to suggest that it is reasonable to think of energy-intensive and nonenergy-intensive goods in this sense (see my paper with Johnson). In any event, the analysis does not depend on the assumption that F and G are negative, only that the capital-labor ratios differ.

II. General Equilibrium Effects

Even in this simple model, the general equilibrium responses engendered by a change in the price of energy are fairly complex. Higher energy prices increase the cost of producing goods, leading to reduced production of one or both outputs. There will also be changes in the composition of output. These have two sources. One is the substitution by both domestic and foreign consumers in response to relative output prices. The other stems from changes in the distribution of income between the domestic economy and the rest of the world. If the derived demand for energy is price inelastic, foreign income will rise as the price of energy rises, and it will fall if energy demand is price elastic. If domestic and foreign consumers have different tastes ($\varepsilon_C \neq \varepsilon_R$), this will have impact on the demands for the two goods. The assumption of homothetic preferences means there will be no change in the consumption bundle of either group as a result of income changes, however.

These changes in the demands for goods will induce changes in the derived demand for inputs, which will depend on relative factor intensities. The interaction of these and the factor supply functions yield factor market equilibrium. Of course, factor ratios in the production of each good are fixed by assumption, but changes in factor ratios for the economy as a whole can occur as the composition of output varies.

As is often the case with general equilibrium models, it is difficult to derive unambiguous results without specific parameter values. For example, even the most intuitive result, that the price of energy-intensive goods should rise relative to the price of nonenergy-intensive goods as the price of energy rises, need not hold.[7] The opposite result can occur if income is transferred from domestic consumers with a strong preference for energy-intensive goods to foreigners with a strong preference for nonenergy-intensive goods. This requires further that the supply of the domestic factor used relatively intensively in nonenergy-intensive goods is sufficiently inelastic to lead to a large increase in the price of that factor, thus driving up the cost of nonenergy-intensive goods.

Likewise, it is not possible to say whether factor prices and factor quantities rise or fall with the price of energy.[8] Although some cases can be ruled out, various sorts of behavior can be produced by choice of parameter values. Nevertheless, despite the outcomes the model is capable of producing, moderate parameter values give rise to the responses one would expect. As the price of energy rises from a low level, foreign income

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[7] The derivative of relative output prices with respect to the price of energy can be obtained from equation (15) via the implicit function theorem:

$$\frac{dP_X}{dP_E}=\frac{-(A_3P_X+A_4+2A_5P_E)}{2A_1P_X+A_2+A_3P_E}.$$

Although A_1 must be nonnegative, the signs of the other coefficients depend on the magnitudes of ε_C and ε_R, and the sign of the expression depends on their relative magnitudes and on prices. Given the normalization, parameter values of $\delta_K=0.001$, $\delta_L=100$, $\alpha_Y=1.0001$, $\beta_Y=7$, $\varepsilon_C=0.95$, and $\varepsilon_R=0.05$ provides a case where dP_X/dP_E is negative for almost half the range of P_E.

[8] For example, if there is sufficient substitution into nonenergy-intensive goods and if the capital-labor ratio in that sector is sufficiently larger than in the production of energy-intensive goods, then the increased demand for capital can bid up the price of capital services relative to the numeraire.

first rises and then falls, while domestic income falls throughout the range of energy prices. The price of energy-intensive goods rises relative to the price of nonenergy-intensive goods, inducing substitution from the former to the latter by both domestic and foreign consumers. Output of both goods is reduced, and equilibrium quantities of both domestic factors fall. Fairly extreme parameter values are required to reverse any of these outcomes.

However, if we look at the response of the aggregate factor ratio in the economy to a change in the price of energy, a different picture emerges. Specifically, consider the aggregate energy-capital and energy-labor ratios; we would describe energy and capital as aggregate substitutes (complements) if the energy-capital ratio falls (rises) in response to higher energy prices, and similarly for energy and labor. How these ratios vary depends on the change in output mix that results from the energy price change,[9] which in turn hinges on the balancing of two potentially opposing effects.

On the one hand, both domestic and foreign consumers substitute out of X goods into Y goods (in the usual case where dP_X/dP_E is positive), which shifts the mix of output toward Y goods. The strength of this effect depends on the difference in sectoral factor intensities; if X goods are much more energy intensive than Y goods (α_Y and β_Y considerably greater than unity), then relative output price changes and substitution by consumers will be larger.

On the other hand, the mix of output shifts toward that good which is purchased relatively more by foreigners, who are becoming relatively more wealthy. If that is Y goods ($\varepsilon_C > \varepsilon_R$), then the mix of output moves unambiguously toward Y goods, and capital and labor will both be aggregate substitutes with energy. If foreigners consume relatively more X goods than domestic consumers ($\varepsilon_C < \varepsilon_R$), however, the two effects work in opposite directions. The strength of the latter effect depends on the difference in consumer preferences. If sectoral factor intensities do not differ greatly, fairly small differences in consumer preferences will cause the composition of output to shift toward X goods. In that case, capital and labor will be aggregate complements with energy.

It should be stressed that both substitutability and complementarity can arise without resort to extreme parameter specifications. While detailed sensitivity analysis is ruled out by the number of parameters, solutions of the model for different specifications provide the following results. If $\varepsilon_R - \varepsilon_C = 0.05$ and the X sector has energy-capital and energy-labor ratios that exceed those in the Y sector by less than 25 percent (α_Y and $\beta_Y < 1.25$), than the effect of income being shifted to foreigners dominates, and energy and capital are complements. If the sectoral factor intensities differ by more than 25 percent, the substitution by consumers away from energy intensive goods dominates, and energy and capital are aggregate substitutes. If $\varepsilon_R - \varepsilon_C = 0.10$, energy and capital are complements (substitutes) if the energy-capital and energy-labor ratios in the X sector exceed those in the Y sector by less (more) than about 50 percent. These results are independent of δ_K and δ_L; recall, too, that if ε_R is less than ε_C, capital and energy are always substitutes.

III. Concluding Remarks

This paper has identified a set of general equilibrium responses to energy price shocks that call into question the interpretation of

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[9] Divide equation (11) by equation (9) to get the aggregate energy-capital ratio, let $X = X_C + X_R$ and $Y = Y_C + Y_R$, and differentiate with respect to P_E to get

$$\frac{d(E/K)}{dP_E} = \frac{(1+\alpha_Y)\left(X\frac{dY}{dP_E} - Y\frac{dX}{dP_E}\right)}{(X+\alpha_Y Y)^2}$$

This derivative is negative if the numerator is negative, which, if we collect terms in dX/dP_E and dY/dP_E and multiply through by P_E/XY, occurs when

$$(P_E/X)(dX/dP_E) < (P_E/Y)(dY/dP_E).$$

These elasticities will typically be negative, although one could be positive. Analogously for the energy-labor ratio.

factor substitution elasticities from aggregate data. Needless to say, there are other possible explanations of the capital-energy complementarity controversy, including those mentioned previously and numerous others. There is no reason that any single explanation should be the correct one; the question is which are empirically important. There is reason to believe the compositional responses to changing relative prices are likely to matter. Differences in energy intensities are large, and various analyses suggest that these responses have occurred or would occur again.

One lesson to take from this analysis is that estimates of factor substitutability based on aggregate data are misleading because they capture more than simply technological substitution. On this view, none of the various empirical measures of factor substitutability is correct; they are not measuring what they want. Factor substitution is a microeconomic phenomenon, and is best examined by looking at microeconomic data. General equilibrium frameworks, of the sort provided by Borges-Goulder or Hudson-Jorgenson, can then serve to integrate the microeconomic aspects in a consistent fashion to permit the examination of energy-economy interactions at a detailed level.

Another way to interpret these results, however, is to argue that the capital-energy complementarity debate, while illuminating issues of model specification and measurement, has been misfocused. Instead of attempting to resolve a dispute about the correct nature of technological factor substitution, energy analysts should recognize that aggregate estimates capture a whole range of economic responses to energy price shocks. While microeconomic results are still called for if our interest is in those aspects, and general equilibrium models are proper if detailed knowledge of responses to price shocks or specific policies is our goal, aggregate analyses may still be of use if our concern is the broad response of the economy to shocks or broad policy measures. Such aggregate analyses need to be applied judiciously; other things are not always equal, so the same event may engender different responses in different times or places. But, of course, the same thing is true of other models as well.

APPENDIX

First, let us solve equations (12) and (13) simultaneously for P_K and P_L as functions of P_X and P_E, making use of the numeraire to eliminate P_Y:

$$P_K = (\beta_Y P_X + FP_E - \beta_X)/D \tag{A1}$$

$$P_L = -(\alpha_Y P_X + GP_E - \alpha_X)/D, \tag{A2}$$

where $D = \alpha_X\beta_Y - \alpha_Y\beta_X$, $F = \beta_X\gamma_Y - \beta_Y\gamma_X$, and $G = \alpha_X\gamma_Y - \alpha_Y\gamma_X$.

I likewise solve equations (9) and (10) to get

$$X_C + X_R = (\beta_Y K - \alpha_Y L)/D \tag{A3}$$

$$Y_C + Y_R = (\alpha_X L - \beta_X K)/D. \tag{A4}$$

I proceed by substituting for K, L, X_C, and X_R in equation (A3), making use of equations (14), (A1), and (A2) to eliminate P_Y, P_K, and P_L where necessary.

I substitute equations (1) and (2) for K and L in equation (A3) to get

$$X_C + X_R = (\beta_Y\delta_K P_K - \alpha_Y\delta_L P_L)/I(\cdot)D. \tag{A5}$$

If I then substitute equations (A1) and (A2) for P_K and P_L and collect terms, we have

$$\begin{aligned} X_C + X_R &= \frac{\beta_Y\delta_K}{I(\cdot)D^2}(\beta_Y P_X + FP_E - \beta_X) \\ &+ \frac{\alpha_Y\delta_L}{I(\cdot)D^2}(\alpha_Y P_X + GP_E - \alpha_X). \end{aligned} \tag{A6}$$

We can derive an analogous expression for the Y goods sector:

$$\begin{aligned} Y_C + Y_R &= -\frac{\beta_X\delta_K}{I(\cdot)D^2}(\beta_Y P_X + FP_E - \beta_X) \\ &- \frac{\alpha_X\delta_L}{I(\cdot)D^2}(\alpha_Y P_X + GP_E - \alpha_X) \end{aligned} \tag{A7}$$

If we substitute equations (1) and (2) for K and L in equation (3), and then substitute for P_K and P_L using equations (A1) and (A2), we get the following expression for X_C:

$$X_C = \frac{\varepsilon_C \delta_K}{I(\cdot)D^2} \frac{(\beta_Y P_X + FP_E - \beta_X)^2}{P_X} + \frac{\varepsilon_C \delta_L}{I(\cdot)D^2} \frac{(\alpha_Y P_X + GP_E - \alpha_X)^2}{P_X} \tag{A8}$$

Substitute equations (A6) and (A7) for $(X_C + X_R)$ and $(Y_C + Y_R)$ in equation (11), and then substitute the result for E in equation (5):

$$X_R = \frac{\varepsilon_R F \delta_K}{I(\cdot)D^2} \frac{(\beta_Y P_X + FP_E - \beta_X)P_E}{P_X} - \frac{\varepsilon_R G \delta_L}{I(\cdot)D^2} \frac{(\alpha_Y P_X + GP_E - \alpha_X)P_E}{P_X}. \tag{A9}$$

Finally, equate the supply of X goods (right-hand side of equation (A6)) to the demand for X goods (sum of the right-hand sides of equations (A8) and (A9)), expand the squared terms, multiply both sides by P_X, and collect terms to get equation (15).[10]

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[10] I have required equilibrium in the markets for capital, labor, energy, and X goods. I can verify that the market for Y goods is also in equilibrium by noting that if we equate the supply of Y goods (equation (A7)) to the demand for Y goods (equations analogous to equations (A8) and (A9)), we also arrive at equation (15).

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Externalities from Contract Length

By LAURENCE BALL*

An increase in the length of a firm's labor contract contributes to rigidity in the aggregate price level. This increases the variance of aggregate demand but decreases the variance of other firms' real wages. Under certain conditions, the net effect is to increase the variance of other firms' employment. This negative externality implies that the equilibrium contract length in a decentralized economy is greater than the social optimum—in other words, wages are too rigid.

In Keynesian macro models, rigidities in nominal wages and prices lead to large deviations of employment and output from efficient levels, and therefore to large welfare losses. As members of the New Classical school point out, these rigidities are difficult to explain. In particular, the costs of making wages and prices less rigid (for example, by adjusting them more frequently or indexing them to more variables) often appear to be small. To take an extreme example, Keynesian economists often refer to the costs of printing new menus or catalogs, which sound trivial. If wage and price rigidities are responsible for large welfare losses, why do wage and price setters refuse to incur the relatively small costs of reducing them?

A possible answer is that keeping wages and prices rigid has negative externalities, because a rigidity created by one agent hurts the overall performance of the economy and therefore reduces the welfare of other agents. This means that small costs of reducing rigidities may exceed the private gains from doing so even though the social gains would be large. This idea sometimes appears in informal discussions of rigidities, such as Charles Schultze's presidential address (1985) and John Taylor (1985). Recently, N. Gregory Mankiw (1985) and others have developed "small menu cost" models that formalize one version of it.[1]

This paper focuses on one frequently cited source of nominal wage rigidity, long-term labor contracts. In particular, I study the choice of contract length, which I interpret as a decision about how rigid to make wages. The costs of shortening labor contracts are clearly greater than trivial menu costs. Shorter contracts require large increases in the amount of time that managers and union officials spend in contract negotiations, and they may lead to more frequent strikes.[2] It seems doubtful, however, that these costs exceed the welfare gains from the decreased magnitude of the business cycle that, according to many Keynesian models, would result from less rigid wages. Therefore, I investigate the possibility that externalities from contract length lead firms and workers to choose long contracts even though shorter ones would be optimal for society.

I study a model of an economy with long-term contracts in the spirit of Jo Anna Gray (1976, 1978), Stanley Fischer (1977), and the recent work of Gary Fethke and Andrew Policano (1984; 1986a, b; 1987). As in those papers, longer contracts reduce contracting costs but increase the variance of employment around the efficient level. This paper, however, addresses a different kind of question from most earlier work. Some previous authors study positive issues, such as why certain types of contracts exist and how they affect the economy. Others solve for the socially optimal choice of various contract provisions, including contract length. In contrast, I study the welfare properties of the contract provisions chosen by firms in a decentralized economy.

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[1] See also George Akerlof and Janet Yellen (1985) and Olivier Blanchard and Nobuhiro Kiyotaki (1985).

[2] Sanford Jacoby and Daniel Mitchell (1984) discuss these and other costs of shorter contracts.

I also depart from previous contract models by assuming that the goods market is monopolistically competitive. In this respect, my model follows the menu cost models of externalities from price rigidity. The role of monopolistic competition in this paper is different from its role in the previous literature, however. In previous papers, monopolistic competition is crucial because it causes socially and privately optimal prices to differ. Here monopolistic competition is crucial because it causes firms' labor demand to depend on aggregate demand as well as the real wage. This leads to externalities from the effects of contract length on aggregate demand.

According to my results, long contracts have both positive and negative externalities. Under certain conditions, the negative externalities are larger, and this implies that firms choose contract lengths that are greater than the social optimum. To understand the argument about externalities at an intuitive level, suppose that one firm out of many, firm *i*, has a contract of some fixed length. Consider the effects on firm *i* of an increase in the contract lengths of *other* firms. Longer contracts for other firms mean that these firms' nominal wages are adjusted less frequently, and this implies that the aggregate wage level responds more slowly to monetary shocks. Since the wage level responds more slowly to shocks, the price level also responds more slowly.

The decreased responsiveness of the price level affects firm *i* in two ways. First, it makes it easier for firm *i* to forecast future prices when it signs contracts specifying future nominal wages. This reduces the variability of firm *i*'s real wage. Second, slower adjustment of prices to monetary shocks means greater variability of real money. This leads to greater variability of aggregate demand, and therefore of the demand for firm *i*'s product.

These changes in the variability of firm *i*'s real wage and product demand affect the variance of its employment around the efficient level, which firm *i* and its workers seek to minimize. Specifically, the decreased variability of the real wage caused by other firms' long contracts *decreases* the variability of firm *i*'s employment, which is determined by labor demand. The increased variability of real money, and hence firm *i*'s product demand, *increases* the variability of firm *i*'s employment. The net effect depends on the relative influence of changes in the real wage and changes in real money on the quantity of labor demanded.

Section I sets up a continuous time model of an economy with long-term contracts. To illustrate the paper's main argument, Section II shows the externalities from contract length in a simplified, discrete time version of the model. In this example, firms choose between one- and two-period contracts. Section III shows the externalities from contract length in the more general model. Section IV compares the contract length that firms choose in a symmetric Nash equilibrium to the socially optimal length. Not surprisingly, if the net externality from long contracts is negative, then the equilibrium length is greater than the social optimum. Section V discusses extensions of the model. Finally, Section VI compares the model to the ones in related papers and offers conclusions.

I. The Model

This section describes a continuous time model of an economy with long-term labor contracts. There is a continuum of firms indexed by *i* and distributed uniformly between $i=0$ and $i=1$. The firms produce differentiated goods. Each firm has a pool of immobile workers. Omitting constants and time indices, firm *i*'s production function at every instant is

$$(1)\qquad y_i=\alpha l_i,\qquad 0<\alpha\leq 1,$$

where y_i is the log of output and l_i is the log of employment.

The money supply is exogenous. The log of money follows a Wiener process (the continuous time analogue of a random walk) with mean zero and forecast variance tV_m conditional on information available at $t=0$. Aggregate money demand is given by

$$(2) \qquad m-p=(1/\sigma)y, \qquad \sigma>0,$$

where $p=\int_{i=0}^{1} p_i\,di$ and $y=\int_{i=0}^{1} y_i\,di$.

m is the log of money, p_i is the log of firm i's product price, and σ is the elasticity of real aggregate demand with respect to real balances. Equation (2) is a generalization of the money demand equation in Gray (1976, 1978), which sets $\sigma=1$.

Demand for firm i's product as a share of aggregate demand depends on i's relative price:

$$(3) \qquad y_i-y=-\varepsilon(p_i-p), \qquad \varepsilon>1.$$

ε is determined by the substitutability of the products of different firms.[3] Combining (2) and (3) yields firm i's product demand:

$$(4) \qquad y_i=\sigma(m-p)-\varepsilon(p_i-p).$$

This is similar to product demand equations in other macro models with monopolistic competition (for example, Julio Rotemberg, 1982). Firm i's demand depends positively on real balances, which affect aggregate demand, and negatively on its relative price.

The production function, the product demand equation, and profit maximization determine the demand for labor:

$$(5) \qquad l_i^D=1/(\alpha+\varepsilon-\alpha\varepsilon) \times[\sigma(m-p)-\varepsilon(w_i-p)],$$

where w_i is the log of firm i's nominal wage. Firm i's labor demand depends positively on real balances and negatively on its real wage. The labor supply of the workers in firm i's labor pool is

$$(6) \qquad l_i^S=\delta(w_i-p), \qquad \delta\geq 0.$$

Labor is sold through long-term contracts similar to the ones in Gray and Fischer. A contract signed at t specifies a contract length, λ, and a path of noncontingent nominal wages from t to $t+\lambda$.[4] Firms choose employment at each instant after observing the current money supply, and so employment is equal to labor demand. Negotiating a contract entails a fixed cost, C, which implies that contracting costs per period are C/λ. An equal number of firms signs contracts at every instant—in other words, there is uniform staggering.[5]

Bargaining between firm i and its workers leads to contract provisions (wages and contract length) that maximize the sum of expected profits and the expected consumers' surplus of the workers, averaged over the life of a contract. Maximizing this sum is the same as minimizing the expected reductions in surplus from two sources: contracting costs; and deviations of employment from the spot market level defined by $l_i^S=l_i^D$. I approximate losses from the latter by $(l_i-l_i^*)^2$, where l_i^* is spot market employment. Thus if firm i signs a contract at time τ, the average of its expected losses is[6]

$$(7) \qquad (1/\lambda)\int_{t=\tau}^{\tau+\lambda} Z_i(t)\,dt+C/\lambda,$$

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[3] $\varepsilon>1$ is necessary for the existence of a profit-maximizing price and output level.

[4] It is common in the contract literature to consider wage contingencies. Since all shocks are nominal, indexation of wages to the price level would keep employment at the spot market level regardless of any firm's contract length; thus contract length would have no externalities. Externalities would also disappear if wages were contingent on the money supply or on employment (as in Robert Hall and David Lilien, 1979, and Fethke and Policano, 1986b). This suggests that contingencies should be analyzed when both real and nominal shocks exist, as in the extension in Section V. (See fn. 16 in that section.)

[5] Uniform staggering simplifies the analysis but is not essential to the main results. The next section contains an example in which signing dates are synchronized, and Section V makes the timing of signing dates endogenous.

[6] Equation (7) is similar to the objective functions in Gray (1976, 1978) and many other papers. $(l_i-l_i^*)^2$ is a second-order Taylor approximation to the losses from deviations of l_i from l_i^* (see my dissertation, 1986). Blanchard (1979) argues that it is reasonable to specify objective functions like (7) if workers are risk neutral in income. (Intuitively, risk-averse workers would care about the variance of real wages as well as the variance of employment.)

where $Z_i(t) = E[(l_i(t) - l_i^*(t))^2]$. The labor supply and labor demand equations imply

$$(8) \qquad l_i^* = \frac{\delta\sigma}{\varepsilon + \delta\alpha + \delta\varepsilon - \delta\alpha\varepsilon}(m-p).$$

II. Externalities: An Example

A. *Assumptions*

This section contains a discrete time example that demonstrates the externalities from contract length as simply as possible. I modify the model described in Section I as follows: m follows a discrete time random walk, and its innovations have mean zero and variance V_m. Firms' production functions exhibit constant returns to scale ($\alpha = 1$). Labor supply is perfectly inelastic ($\delta = 0$), which implies that l_i^* is always equal to zero. Since $l_i^* = 0$, Z_i is simply the variance of log employment around zero.[7]

Firms choose between two possible contract lengths: one period and two periods. If firm i signs one-period contracts, then at every t, it sets w_{it}. If it signs two-period contracts, then at every other t, it sets w_{it} and w_{it+1}. When firms sign contracts at t, they know m_t, the current money supply. All firms that sign two-period contracts sign them in the same periods—in other words, signing dates are synchronized.

B. *The Behavior of the Price Level*

The first step in studying externalities is to determine how contract length affects the behavior of the price level. I solve for the price level assuming that all firms sign one-period contracts and assuming that all sign two-period contracts. In both regimes, combining the production function with the product and labor demand equations and aggregating across firms yields

$$(9) \qquad p_t = w_t,$$

where $$w_t = \int_{i=0}^{1} w_{it}\,di.$$

Therefore, I solve for wages in the two regimes.

One-period contracts: In this regime, firms sign contracts specifying current wages after observing the current money supply. This implies that firms are able to choose wages that set $l_{it} = l_{it}^* = 0$. In other words, one-period contracts can replicate a spot labor market. The labor demand equation implies that $l_{it} = 0$ when

$$(10) \qquad w_{it} = \frac{\sigma}{\varepsilon}m_t + \left(1 - \frac{\sigma}{\varepsilon}\right)p_t.$$

Combining this with (9) and aggregating implies

$$(11) \qquad p_t = m_t.$$

Two-period contracts: When firms sign synchronized two-period contracts, they set wages for the current period and the next period. For the current period, they once again use (10) to set $l_{it} = 0$. Therefore, the behavior of p_t in the first period of the contracts is the same as its behavior in the one-period regime:

$$(12) \qquad \textit{First period}: \; p_t = m_t.$$

If t is the second period of the contracts, then second-period wages, w_{it}, are chosen at $t-1$. Minimization of Z_i implies that w_{it} is chosen to set

$$(13) \qquad E_{t-1}[l_{it}] = 0.$$

Substituting the labor demand equation for l_{it} and rearranging yields

$$(14) \quad w_{it} = \frac{\sigma}{\varepsilon}E_{t-1}[m_t] + \left(1 - \frac{\sigma}{\varepsilon}\right)E_{t-1}[p_t].$$

Combining this with (9) and aggregating

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[7] As pointed out in fn. 6, Blanchard assumes that workers are risk neutral in income in justifying objective functions like (7). Risk neutrality implies a strictly positive labor supply elasticity, because the income effect is zero. Thus we should think of $\delta = 0$ as a limiting case rather than simply a special case. The more general model of the next section allows $\delta > 0$.

TABLE 1—COMPUTING FIRM i'S LOSS IN SECOND PERIOD OF TWO-PERIOD CONTRACT

	If All Other Firms Have One-Period Contracts	If All Others Have Two-Period Contracts[a]
$m_t - p_t$	$m_t - m_t = 0$	$m_t - m_{t-1} = \Delta m_t$
$w_{it} - p_t$	$m_{t-1} - m_t = -\Delta m_t$	$m_{t-1} - m_{t-1} = 0$
$l_{it} = \sigma(m_t - p_t)$ $- \varepsilon(w_{it} - p_t)$	$\varepsilon(\Delta m_t)$	$\sigma(\Delta m_t)$
$Z_{it} = E[l_{it}^2]$	$\varepsilon^2 V_m$	$\sigma^2 V_m$

Note: Longer contracts for other firms raise Z_i if and only if $\sigma > \varepsilon$.

[a] Signed in the same periods as firm i's contracts.

leads to

$$(15)\quad p_t = \frac{\sigma}{\varepsilon} E_{t-1}[m_t] + \left(1 - \frac{\sigma}{\varepsilon}\right) E_{t-1}[p_t].$$

Finally, substituting m_{t-1} for $E_{t-1}[m_t]$, because m follows a random walk, and manipulating expectations yields

$$(16)\quad \textit{Second period: } p_t = m_{t-1}.$$

This example illustrates the key effect of an increase in contract length on the behavior of the economy: reduced responsiveness of the price level to monetary shocks. If firms sign one-period contracts, the price level always equals the money supply. If they sign two-period contracts, then, in the second period, prices have not adjusted to the most recent shock.

C. *Externalities*

Define the externalities from contract length as the effects on firm i of a change in the contract lengths of *other* firms. Specifically, suppose that firm i's contract length is fixed. If all other firms change from one-period contracts to two-period contracts, how does this affect Z_i?

The answer depends on the length of firm i's contracts. If firm i signs one-period contracts, then it can always use the wage-setting rule, (10), to set $l_{it} = 0$. Therefore, $Z_i = 0$ regardless of other firms' contract lengths, which means that there is no externality.[8] If firm i signs two-period contracts, then there is no externality in the first period because firm i can again use (10) to set $l_{it} = 0$. Thus externalities exist in this example only if firm i is in the second period of a two-period contract. The remainder of this section considers this situation.

Table 1 shows how to derive Z_i in the second period of firm i's contract for each of the two possible lengths of other firms' contracts. If the other firms choose two periods, then their contracts are synchronized with firm i's. Therefore, they are also in the second period.

Firm i is only one of a continuum of firms, and so its contract length has no effect on the behavior of the price level. Therefore, $p_t = m_t$ if the other firms have one-period contracts (equation (11)), and $p_t = m_{t-1}$ if the others have two-period contracts, since the contracts are in the second period (equation (16)). Given these formulae for the price level, Table 1 shows real money and firm i's real wage as functions of the monetary shocks. The derivation of the real wage uses firm i's rule for setting second-period nominal wages, (14), and the fact that $E_{t-1}[m_t] = m_{t-1}$. Substituting real money and the real wage into labor demand yields firm i's employment and $Z_i = E[l_i^2]$. An increase in other firms' contract lengths raises Z_i if and only if $\sigma > \varepsilon$.

The explanation for this result is the following. As noted above, an increase in other firms' contract lengths makes the price level less responsive to monetary shocks. This has two effects. First, it makes real money more responsive to shocks. In this example, real money is constant if other firms sign one-period contracts; real money varies in response to shocks if other firms sign two-period contracts. Second, since firm i's nominal wage is fixed in advance, the reduced responsiveness of the price level to shocks makes firm i's real wage less responsive. In this example, firm i's real wage is constant if other firms sign two-period contracts.

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[8] Other firms' lengths affect the behavior of the economy, but with one-period contracts, firm i can always adjust its wage to keep its employment at the spot market level. This "no externality" case exists because contract length is discrete. In the continuous time model of the next section, other firms' lengths affect Z_i regardless of firm i's length.

The increased responsiveness of real money to monetary shocks increases the variance of firm i's labor demand, which determines its employment. The decreased responsiveness of firm i's real wage decreases the variance of its labor demand. The net effect depends on the sizes of the effects of real money and the real wage on labor demand. The labor demand equation shows that these depend on the sizes of σ and ε.

An analogy to the Malinvaud model makes this result easier to interpret (Edmond Malinvaud, 1977). In Malinvaud, changes in real wages affect employment but changes in real money do not when unemployment is "classical." The reverse is true when unemployment is "Keynesian." In this model, we can think loosely of unemployment as partly Keynesian and partly classical, with the sizes of σ and ε determining which polar case is closer. Long contracts have a negative net externality if unemployment is more Keynesian.

Recall that σ is the elasticity of firm i's product demand with respect to real money. This is equal to the elasticity of aggregate demand with respect to real money, which is the inverse of the income elasticity of money demand. ε is the elasticity of firm i's product demand with respect to its relative price. *Ceterus paribus*, strong effects of real money on aggregate demand mean that increases in other firms' contract lengths are likely to raise Z_i. A large price elasticity of firm i's product demand implies the opposite.[9]

III. Externalities from Contract Length: The More General Model

This section studies the externalities from contract length in the continuous time model of Section I. To review, log money follows a Wiener process; a contract signed at t specifies a contract length, λ, and a path for wages from t to $t+\lambda$; signing dates are uniformly staggered; and the assumptions of constant returns to scale and inelastic labor supply are relaxed. The condition for a negative net externality from long contracts is similar, although not identical, to the condition in the previous section.

A. *The Behavior of the Price Level*

As in the previous section, the first step is to determine how contract length affects the behavior of the price level. I assume that all firms have contracts of some fixed length, λ, and solve for the price level as a function of λ and the innovations in money.

Combining equations (1), (4), and (5) yields the price level as a function of the money supply and aggregate wages:

$$(17)\quad p(t)=\beta w(t)+(1-\beta)m(t)$$

where $\beta=\alpha/(\alpha+\sigma-\alpha\sigma), \quad 0<\beta\leq 1.$

To solve for $p(t)$ as a function of the innovations in money, I derive firms' wage-setting rules. If firm i sets $w_i(t)$ at τ, minimization of $Z_i(t)$ requires

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[9] Recall that $\varepsilon>1$ is necessary to assure that firms choose finite product prices. $\sigma>1$ holds as long as the income elasticity of money demand is less than one, which is an empirically reasonable assumption. (See, for example, Stephen Goldfeld, 1973.) It is probably a mistake, however, to try to assign numbers to the price elasticity of product demand and the income elasticity of money demand to determine the sign of the net externality. The result that this sign depends on the elasticities of labor demand with respect to the real wage and real money is, I believe, robust. The precise relation of these labor demand elasticities to the parameters of the money and product demand equations depends on my log-linear specification. Another problem with assigning values to σ and ε is that the model does not permit distinctions between short- and long-run elasticities, which may be important empirically. It is possible, for example, that the elasticity of a firm's product demand with respect to cyclical fluctuations in its price is less than one even though the elasticity with respect to long-run price changes is greater than one. This might be true if permanent price increases drive customers away but temporary increases do not.

$$(18) \qquad E_\tau[l_i(t) - l_i^*(t)] = 0.$$

Substituting the labor demand equation and the equation for l_i^*, (8), into (18) and solving for w_i yields

$$(19) \quad w_i(t) = Rm(\tau) + (1-R)E_\tau p(t),$$

$$\text{where} \quad R = \sigma/(\varepsilon + \delta\alpha + \delta\varepsilon - \delta\alpha\varepsilon), \quad R > 0.$$

The derivation of (19) uses the fact that $E_\tau m(t) = m(\tau)$ because m follows a Wiener process without drift.

Recall that contract signings are uniformly staggered. This implies that $w(t)$ is the average of wages set at every instant between t and $t-\lambda$, the signing date of the contracts that expire at t. The Appendix combines this fact with (17) and (19) to derive $p(t)$ as a function of $m(t-\lambda)$ and innovations in m between $t-\lambda$ and t:

$$(20) \quad p(t) = m(t-\lambda) + \int_{s=-\lambda}^{0} \gamma(s,\lambda)\, dm\,(t+s)$$

$$\text{where} \quad \gamma(s,\lambda) = \frac{\lambda - \beta sR - \beta\lambda}{\lambda - \beta sR + \beta s}, \qquad 1-\beta \le \gamma \le 1.$$

The second term on the right side of (20) is a stochastic integral. (See Paul Hoel et al., 1972). $\gamma(s,\lambda)$ is a weighting function that shows the effects on $p(t)$ of unit innovations in m between $t-\lambda$ and t. The function γ has the following properties:

1) $\gamma < 1$ for a shock after $t-\lambda$. At t, not all firms have adjusted their wages since the shock, and so the price level has not adjusted fully.

2) $\partial\gamma/\partial s < 0$. A more recent shock has a smaller effect on $p(t)$ than a less recent one because fewer firms have adjusted their wages since it occurred.

3) Most important, $\partial\gamma/\partial\lambda < 0$ for $-\lambda < s < 0$. Longer contracts decrease the responsiveness of $p(t)$ to a shock at a fixed point in time between $t-\lambda$ and t. This is because longer contracts mean that fewer firms adjust their wages between the occurrence of the shock and t.

B. *Externalities*

As in the discrete time example, I show how firm i's loss function is affected by an increase in other firms' contract lengths. Assume that firm i chooses a fixed length, λ', and all other firms choose length λ. Since firm i is only one firm, the price level behaves as though all firms chose λ. How does an increase in λ affect Z_i?[10]

As in the example, Z_i depends on the behavior of real money and firm i's real wage. The Appendix uses (20) and the wage-setting rule (19) to derive these variables. Substituting them into the labor demand equation and the equation for l_i^*, (8), yields Z_i:

$$\begin{aligned}(21) \quad Z_i(t) &= E\left[(l_i(t) - l_i^*(t))^2\right] \\ &= V_m \int_{s=\tau-t}^{0} [a_1(1-\gamma) + a_2\gamma]^2\, ds \\ &\qquad\qquad \text{for } \tau \ge t-\lambda \\ &= V_m \int_{s=-\lambda}^{0} [a_1(1-\gamma) + a_2\gamma]^2\, ds \\ &\quad + V_m a_2^2 (t-\tau-\lambda) \\ &\qquad\qquad \text{for } \tau < t-\lambda\end{aligned}$$

$$\text{where} \quad a_1 = \frac{\sigma}{\alpha + \varepsilon - \alpha\varepsilon} - \frac{\delta\sigma}{\varepsilon + \delta\alpha + \delta\varepsilon - \delta\alpha\varepsilon}, \qquad a_2 = \frac{\varepsilon}{\alpha + \varepsilon - \alpha\varepsilon}$$

and where τ is the signing date of firm i's contract, $\tau \leq t$.[11] Recall that $\partial\gamma/\partial\lambda < 0$. This implies that

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[10] My dissertation discusses a complication that I ignore here. If the labor supply elasticity is nonzero, then the effect of λ on $Z_i = E[(l_i - l_i^*)^2]$ is not the only externality. λ also affects the sum of firm i's expected profits and the expected consumers' surplus of its workers *conditional on* $l_i = l_i^*$. See ch. III, Appendix B, for a description of this subtle effect and an argument that it is probably unimportant.

$$(22) \qquad dZ_i(t)/d\lambda \gtreqless 0 \quad \text{if } \sigma \gtreqless \varepsilon + \delta.$$

According to (22), an increase in contract length has a negative net externality if $\sigma > \varepsilon + \delta$. For the special case of $\delta = 0$, this result is the same as the one in the discrete time example, which assumed $\delta = 0$. For this special case, the explanation is also the same. Longer contracts decrease the responsiveness of the price level to monetary shocks ($\partial\gamma/\partial\lambda < 0$). This increases the responsiveness of real money to shocks but decreases the responsiveness of firm i's real wage. The elasticities of labor demand with respect to these two variables determine the net externality.

When the labor supply elasticity is nonzero, the explanation for (22) is more complicated. Equation (22) shows that a large positive δ makes it more likely that an increase in λ *reduces* $Z_i = E[(l_i - l_i^*)^2]$. To understand this, recall from equations (5) and (8) that an increase in real money raises both firm i's actual employment, l_i, and its spot market employment, l_i^*. The increase in l_i^* is smaller, but it partially offsets the effect of the higher l_i on $l_i - l_i^*$. A large δ means that fluctuations in real money cause large fluctuations in l_i^*, and therefore small fluctuations in $l_i - l_i^*$. If fluctuations in real money cause only small fluctuations in $l_i - l_i^*$, then the channel through which increases in λ raise Z_i—greater fluctuations in real money—is unimportant. As a result, a large δ makes it more likely that the net effect of an increase in λ is to decrease Z_i.[12]

IV. Equilibrium and Optimal Contracts

This section compares the socially optimal contract length to the length that firms choose in a symmetric Nash equilibrium. I analyze the continuous time version of the model. It is impossible to solve explicitly for the equilibrium and optimal lengths, and so I compare first-order conditions that define them implicitly. Not surprisingly, if the net externality from increases in contract length is negative, then equilibrium contracts are longer than optimal contracts.

Following Gray, define the optimal length to be the one that, if adopted by all firms, minimizes firms' loss functions. From (7), the optimal length is the solution to

$$(23) \qquad \min_{\lambda} \frac{1}{\lambda}\int_{t=\tau}^{\tau+\lambda} Z_i(t)\,dt + \frac{C}{\lambda},$$

where Z_i is given by (21). The first-order condition is

$$(24) \qquad \psi(\lambda) = C,$$

$$\text{where} \quad \psi(\lambda) = \lambda Z_i(\tau+\lambda) + \lambda\int_{t=\tau}^{\tau+\lambda}\frac{dZ_i(t)}{d\lambda}\,dt - \int_{t=\tau}^{\tau+\lambda} Z_i(t)\,dt.$$

Now consider the decentralized equilibrium. One firm's choice of contract length depends on other firms' lengths, because these affect the behavior of the economy. I study firm i's problem of choosing its length, λ', given that all other firms have length λ. A symmetric Nash equilibrium is a fixed point of $\lambda'(\lambda)$.

Firm i's problem is

$$(25) \qquad \min_{\lambda'} \frac{1}{\lambda'}\int_{t=\tau}^{\tau+\lambda'} Z_i(t)\,dt + \frac{C}{\lambda'},$$

where Z_i is again given by (21). Note that $Z_i(t)$ depends on λ, which affects the behavior of the economy, but not on λ'. Differentiating (25) yields the first-order condition

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[11] $\tau < t - \lambda$ means that firm i's contract was signed before the oldest contract in the rest of the economy. This is possible if firm i's contract is longer than the contracts of the other firms ($\lambda' > \lambda$).

[12] Intuitively, the role of fluctuations in l_i^* in determining the externalities from contract length seems less important than the role of fluctuations in actual employment. Formally, this is the case if δ is small, because a small labor supply elasticity implies that l_i^* is close to a constant. Most empirical evidence suggests that labor supply elasticities are near zero, at least for prime-age men, who make up the bulk of workers covered by long-term contracts. (See Mark Killingsworth, 1983, for a survey of labor supply elasticity estimates.)

$$\lambda' Z_i(\tau + \lambda') - \int_{t=\tau}^{\tau+\lambda'} Z_i(t)\,dt = C. \tag{26}$$

A fixed point of $\lambda'(\lambda)$ is defined by setting $\lambda' = \lambda$ in (26). Thus the equilibrium length is the solution to[13]

$$\phi(\lambda) = C, \tag{27}$$

$$\text{where} \quad \phi(\lambda) = \lambda Z_i(\tau + \lambda) - \int_{t=\tau}^{\tau+\lambda} Z_i(t)\,dt.$$

The Appendix uses (24) and (27) to show that the equilibrium length is greater than the optimal length if and only if long contracts have negative externalities ($\partial Z_i / \partial \lambda > 0$). The intuition is straighforward. In equilibrium, the gains to a firm from a small decrease in its contract length equal the added contracting costs. If long contracts have negative externalities, then the social gains from a shorter contract are greater than the gains to the firm, and therefore greater than the added contracting costs. Thus reducing contract length below the equilibrium raises welfare.

V. Extensions

This section sketches two natural extensions of the model. I discuss results intuitively and omit general proofs.

A. *Endogenous Timing of Contract Signings*

Suppose that the timing of contract signings is endogenous. Rather than imposing either synchronization or staggering, assume that each firm chooses when to sign contracts given other firms' timing, and consider the resulting Nash equilibrium. This can be done for the discrete time example following the approach of Fethke and Policano (1986a). With this modification of the example, one can establish two results. First, the condition for a negative externality from long contracts is $\sigma > \varepsilon$ regardless of the equilibrium timing. This is not surprising, since $\sigma > \varepsilon$ is the condition, given inelastic labor supply, when I impose either synchronization (in Section II) or staggering (in Section III). Second, $\sigma > \varepsilon$ implies that the equilibrium timing of two-period contracts is uniform staggering; when $\sigma < \varepsilon$, the equilibrium is synchronization.[14] Thus staggering is the equilibrium if and only if long contracts have negative externalities.

To understand the second result, recall the example. If other firms choose one-period contracts, then they adjust their wages in the middle of firm i's contracts, when i's wage is fixed. Long contracts have negative net externalities if firm i benefits from this adjustment. Suppose that this is the case. Even if all firms choose two-period contracts, firm i can cause others to adjust in the middle of its contract by staggering its signing dates with theirs. Of course, if firm i can gain by staggering, then so can other firms. Thus the equilibrium timing is uniform staggering.

On the other hand, suppose that long contracts have positive net externalities. This means that a firm is hurt if others adjust their wages in the middle of its contract. In this case, if firms have synchronized two-period contracts, then no firm has an incentive to change its timing. Thus synchronization is an equilibrium.

B. *Real Shocks*

Following previous contract models, suppose that a shock is added to the production function:

$$y_i = \alpha l_i + \theta, \tag{28}$$

where θ is stochastic. This modification does not affect the condition for a negative net externality from long contracts. In fact, the condition is the same even if θ is the *only* shock—slow adjustment of wages to real shocks has the same externalities as slow adjustment to monetary shocks. The Appendix establishes these results for the example of Section II.[15,16]

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[13] Tedious manipulations establish that $\lim_{\lambda \to 0} \phi(\lambda) = 0$, $\lim_{\lambda \to \infty} \phi(\lambda) = \infty$, and $d\phi/d\lambda > 0$. These results imply that (27) holds for exactly one λ. Therefore, a unique equilibrium exists.

[14] More precisely, if $\sigma < \varepsilon$, then both synchronization and staggering are equilibria, but only synchronization is stable.

VI. Discussion and Conclusions

A. *Comparison to Other Literature*

Informal Discussions of Externalities: My argument that long contracts may have negative externalities is in the spirit of previous informal discussions of wage rigidities. For example, Schultze argues:

> The major gains, in terms of higher employment, that [come] from lowering wages in the face of downward aggregate demand shocks do not accrue to particular workers in particular firms as a result of their own actions. Rather, the gains accrue through the effects of generalized lower wages in reducing prices, raising real money balances and thereby increasing aggregate demand. ...The large comparative statics gain from an aggregate nominal wage cut does not translate into such a gain seen from the point of view of individual firms. [p. 12]

In both Schultze's analysis and mine, wage flexibility has positive externalities because it leads to changes in the price level, and hence real balances, that reduce fluctuations in aggregate demand.

The Gray/Fischer Model: This paper departs from previous contract models by assuming monopolistic competition in the goods market. This is crucial to the possibility of negative externalities from long contracts. The Gray/Fischer assumption of perfect competition is a limiting case of my model in which $\varepsilon \rightarrow \infty$ (i.e., firms' product demand curves are perfectly elastic). According to (22), $\varepsilon \rightarrow \infty$ implies a positive net externality from increases in contract length.

To understand why, consider the labor demand equation when $\varepsilon \rightarrow \infty$.

$$(29) \qquad l_i^D = -(1/(1-\alpha))(w_i - p).$$

Firm i's labor demand depends on its real wage but not on real money. The reason is that a perfect competitor can sell any quantity of output at the going price regardless of the state of the economy. Therefore, he cares about the relative price of goods and labor, but not about aggregate demand. Because only the real wage matters for labor demand, the only externality from increases in contract length is the positive one of reducing the variance of real wages.

The "Menu Cost" Literature: The work of Mankiw, Akerlof-Yellen, and Blanchard-Kiyotaki has the same broad purpose as this paper: to show that the social costs of wage and price rigidities may be greater than the private costs. Their models are similar to mine in that monopolistic competition is the key to the results. The role of monopolistic competition is very different, however. In the small menu cost models, imperfect competi-

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[15] Adding real shocks complicates the explanation of the externalities. For example, if only real shocks exist, then long contracts *decrease* the variance of real money around its mean. However, they increase the variance of real money around the level that keeps aggregate employment constant. Productivity shocks cause this level to vary.

[16] When both real and nominal shocks exist, indexation of wages to the price level is not sufficient to keep employment at the spot market level. Chapter IV of my dissertation adds indexation to a one-period version of the two-shock model and studies the externalities from firms' choices of the degree of indexation. Future research should investigate the externalities from indexation and contract length when both are endogenous. No matter how many shocks exist, indexation of wages to all shocks implies that contracts replicate spot markets, and therefore that contract length has no externalities. Thus this paper, like the previous contract literature, relies on the impossibility of complete contingencies. This can be justified if some shocks are not observed by all parties to contracts (see Fethke and Policano, 1984). Note that monopolistic competition complicates this justification. Under perfect competition, it is reasonable to assume that firms and workers do not observe nominal aggregate demand, which is equal to the money supply in this paper. Under monopolistic competition, however, firms can infer the money supply from the aggregate price level and their individual price and sales (see the product demand equation, (4)). It is still plausible to assume that workers, who do not directly observe sales, cannot infer aggregate demand and that this precludes complete contingencies.

tion is crucial because it means that the levels of output that are optimal for wage and price setters are lower than the levels that are optimal for society. Therefore, starting from an equilibrium in which prices are optimal for price setters, nonadjustment of prices to a negative monetary shock imposes a first-order loss on society but a second-order loss on price setters by the envelope theorem.

In this paper, monopolistic competition is crucial because it means that aggregate demand affects firms' labor demand. The inefficiency of monopolistic competition in the absence of shocks is *not* important. To see this, suppose that firms act competitively, setting price equal to marginal cost despite facing downward-sloping demand curves. The no-shock levels of employment and output are Pareto optimal, but the externalities from contract length do not disappear. In fact, the only change in the model is an increase in the constant in the labor demand equation (which is omitted in the text).[17] The effects of other firms' contract lengths on the variance of firm i's employment around the intersection of l_i^S and l_i^D are the same.

Fethke and Policano: Recall that the equilibrium timing of contract signings is uniform staggering if and only if long contracts have negative net externalities. As this suggests, my results are related to Fethke and Policano's (1984; 1986a, b; 1987) results about timing. In particular, they show that staggering increases the responsiveness of a firm's real wage to aggregate shocks. This undesirable effect of staggering—that is, of adjustment of some firms' wages while others' wages are fixed—is precisely the positive externality from long contracts.

Note two important differences between Fethke and Policano's models and mine. First, equilibrium timing in Fethke-Policano is determined largely by the importance of sector-specific shocks, which I ignore.[18] Second, they always assume either perfect competition ($\varepsilon \rightarrow \infty$ in my notation) or a unit income elasticity of money demand ($\sigma = 1$). Thus σ is always less than ε. In my model, this would imply that long contracts have positive net externalities; the analogue in Fethke and Policano is that wage setting is synchronized if all shocks are aggregate.

B. *Conclusions*

Long contracts have both positive and negative externalities. Firm i and its workers are affected by other firms' contract lengths because longer contracts make wages, and hence the price level, less responsive to monetary shocks. This increases the variability of aggregate demand, which destabilizes firm i's employment. It also decreases the variability of firm i's real wage, which stabilizes firm i's employment. The net effect depends on the relative influence of changes in real money and changes in the real wage on the quantity of labor demanded.

If the net externality from increases in contract length is negative, then the length that firms choose in equilibrium is greater than the socially optimal length. A government regulation requiring shorter contracts would increase welfare, because the gains from more stable employment would outweigh the increased contracting costs.[19]

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[17] Let the constants in the production function and product demand equation be zero. Profit maximization by firm i implies

$$l_i^D = \varepsilon/(\alpha+\varepsilon-\alpha\varepsilon)\ln((\alpha\varepsilon-\alpha)/\varepsilon) + (1/(\alpha+\varepsilon-\alpha\varepsilon))[\sigma(m-p)-\varepsilon(w_i-p)].$$

Price equal marginal cost implies

$$l_i^D = \varepsilon/(\alpha+\varepsilon-\alpha\varepsilon)\ln(\alpha) + (1/(\alpha+\varepsilon-\alpha\varepsilon))[\sigma(m-p)-\varepsilon(w_i-p)].$$

[18] Fethke-Policano (1986a) show that the role of relative shocks depends on the sizes of sectors of the economy. If there are a few large sectors and firms within a sector receive the same shocks, then relative shocks create an incentive to stagger. If there are many sectors, relative shocks do not affect the results. If relative shocks were added to my model, it is likely that their role would similarly depend on the sizes of sectors.

[19] Barry Bosworth (1981) and Lester Thurow (1982) propose regulation of contract length. Jacoby and Mitchell raise objections.

Many economists argue that wage rigidities caused by long labor contracts could not be an important source of inefficient fluctuations in employment. If they were, claims this school, the parties to contracts would reduce these rigidities, since the costs of doing so seem small compared to the costs of employment fluctuations. The model in this paper suggests a response: While the gains to society from making a firm's wages less rigid are large, the gains to the parties who set the firm's wages may be smaller. Therefore, it may be rational for each firm to maintain rigidities even if reducing them would increase social welfare.

APPENDIX

A. *Derivation of* (20) *and* (21)

As noted in the text, the aggregate wage in the continuous time model is the average of wages set at every instant between $t-\lambda$ and t. Using (19),

$$\text{(A1)}\quad w(t)=\frac{1}{\lambda}\int_{s=-\lambda}^{0}\left[Rm(t+s)+(1-R)E_{t+s}p(t)\right]ds.$$

Combining (A1) with (17) yields

$$\text{(A2)}\quad p(t)=\frac{\beta}{\lambda}\int_{s=-\lambda}^{0}\left[Rm(t+s)+(1-R)E_{t+s}p(t)\right]ds+(1-\beta)m(t).$$

To derive (20), guess the general form and then solve for $\gamma(s,\lambda)$. To solve for γ, substitute (20) into (A2) to obtain

$$\text{(A3)}\quad p(t)=\frac{\beta}{\lambda}\int_{s=-\lambda}^{0}\Big[Rm(t+s)+(1-R)m(t-\lambda)+(1-R)\times\int_{x=-\lambda}^{s}\gamma(x,\lambda)\,dm(t+x)\Big]ds+(1-\beta)m(t).$$

Replace $m(t)$ with $m(t-\lambda)+\int_{s=-\lambda}^{0}dm(t+s)$ and replace $m(t+s)$ with $m(t-\lambda)+\int_{x=-\lambda}^{s}dm(t+x)$. Then manipulate the result to obtain

$$\text{(A4)}\quad p(t)=m(t-\lambda)+\frac{\beta}{\lambda}\int_{s=-\lambda}^{0}\int_{x=-\lambda}^{s}\left[R+(1-R)\gamma(x,\lambda)\right]dm(t+x)\,ds+(1-\beta)\int_{s=-\lambda}^{0}\times dm(t+s).$$

Integration by parts yields[20]

$$\text{(A5)}\quad p(t)=m(t-\lambda)+\beta\int_{s=-\lambda}^{0}(-s/\lambda)\left[R+(1-R)\gamma(s,\lambda)\right]dm(t+s)+(1-\beta)\int_{s=-\lambda}^{0}dm(t+s),$$

$$\text{(A6)}\quad p(t)=m(t-\lambda)+\int_{s=-\lambda}^{0}\{(-s\beta/\lambda)\left[R+(1-R)\gamma(s,\lambda)\right]+(1-\beta)\lambda\}\,dm(t+s).$$

Setting γ equal to the coefficient on dm in (A6) and solving for γ yields the expression in the text.

To derive (21), first solve for real money and firm i's real wage. Note that $m(t)=m(t-\lambda)+\int_{s=-\lambda}^{0}dm(t+s)$. Combining this with (20) yields real money:

$$\text{(A7)}\quad m(t)-p(t)=\int_{s=-\lambda}^{0}\left[1-\gamma(s,\lambda)\right]dm(t+s).$$

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[20] The stochastic integrals in (A4) can be interpreted as normal Riemann-Stieltjes integrals because the coefficients on *dm* are non-stochastic. Thus standard integration by parts is valid. See Hoel et al.

Firm i's real wage is derived from (20) and the wage-setting rule, (19).

$$\text{(A8)}\quad w_i(t)-p(t)$$

$$=R\int_{s=-\lambda}^{\tau-t}(1-\gamma)\,dm\,(t+s)$$

$$-\int_{s=\tau-t}^{0}\gamma\,dm\,(t+s)\quad \text{for } \tau\geq t-\lambda$$

$$=-\int_{s=\tau-t}^{-\lambda}dm\,(t+s)$$

$$-\int_{s=-\lambda}^{0}\gamma\,dm\,(t+s)\quad \text{for } \tau< t-\lambda$$

where τ is the signing date of firm i's contract.

The labor demand equation and the equation for l_i^*, (8), imply

$$\text{(A9)}\quad l_i(t)-l_i^*(t)=a_1[m(t)-p(t)]$$

$$-a_2[w_i(t)-p(t)],$$

where a_1 and a_2 are defined in the text. Substituting (A7) and (A8) into (A9) yields

$$\text{(A10)}\quad l_i(t)-l_i^*(t)$$

$$=\int_{s=\tau-t}^{0}[a_1(1-\gamma)+a_2\gamma]\,dm\,(t+s)$$

$$\text{for } \tau\geq t-\lambda$$

$$=\int_{s=-\lambda}^{0}[a_1(1-\gamma)+a_2\gamma]\,dm\,(t+s)$$

$$+\int_{s=\tau-t}^{-\lambda}a_2\,dm\,(t+s)$$

$$\text{for } \tau< t-\lambda.$$

Equation (21) follows from (A10).

B. *Comparison of Equilibrium and Optimal Contract Lengths*

Call the optimal contract length λ^0 and call the equilibrium length λ^e. We can determine which is larger by comparing (24) and (27). The definitions of ψ and ϕ imply

$$\text{(A11)}\quad \psi(\lambda)=\phi(\lambda)+\lambda\int_{t=\tau}^{\tau+\lambda}\frac{dZ_i(t)}{d\lambda}\,dt.$$

Suppose that $dZ_i(t)/d\lambda>0$. Then $\psi(\lambda)>\phi(\lambda)$. By the definitions of λ^0 and λ^e, $\psi(\lambda^0)=\phi(\lambda^e)=C$. $\psi(\lambda^0)>\phi(\lambda^0)$, so $\phi(\lambda^e)>\phi(\lambda^0)$. ϕ is strictly increasing, so $\lambda^e>\lambda^0$. Thus if the net externality from long contracts is negative ($\partial Z_i/\partial\lambda>0$), then the equilibrium length is greater than the optimal length. By a similar argument, $dZ_i/d\lambda<0$ implies $\lambda^e<\lambda^0$.

C. *Real Shocks*

This part of the Appendix adds a productivity shock to the example in Section II. Replace the production function (1) with (28) and assume that θ follows a random walk. Innovations in θ have mean zero and variance V_θ. The labor demand equation becomes

$$\text{(A12)}\quad l_i^D=\sigma(m-p)-\varepsilon(w_i-p)$$

$$+(\varepsilon-1)\theta.$$

(Recall that $\alpha=1$ in the example.) The price level is derived in the same way as in the text:

$$\text{(A13)}\quad \text{One-period contracts: } p_t=m_t-\frac{1}{\sigma}\theta_t,$$

Two-period contracts:

$$\text{First period: } p_t=m_t-\frac{1}{\sigma}\theta_t$$

$$\text{Second period: } p_t=m_{t-1}+\left(1-\frac{1}{\sigma}\right)\theta_{t-1}-\theta_t.$$

Firm i's rule for setting second-period wages is

$$\text{(A14)}\quad w_{it}=\frac{\sigma}{\varepsilon}E_{t-1}m_t+\left(1-\frac{\sigma}{\varepsilon}\right)E_{t-1}p_t$$

$$+\left(1-\frac{1}{\varepsilon}\right)E_{t-1}\theta_t.$$

TABLE 2—FIRM i'S SECOND-PERIOD LOSS WHEN BOTH REAL AND NOMINAL SHOCKS EXIST

	If All Other Firms Have One-Period Contracts	If All Others Have Two-Period Contracts
$m_t - p_t$	$\frac{1}{\sigma}\theta_t$	$m_t - m_{t-1} + \theta_t - \left(1 - \frac{1}{\sigma}\right)\theta_{t-1}$
$w_{it} - p_t$	$m_{t-1} - m_t + \left(1 - \frac{1}{\sigma}\right)\theta_{t-1} + \frac{1}{\sigma}\theta_t$	θ_t
$l_{it} = \sigma(m_t - p_t) - \varepsilon(w_{it} - p_t) + (\varepsilon - 1)\theta_t$	$\varepsilon(\Delta m_t) + \left(\varepsilon - \frac{\varepsilon}{\sigma}\right)(\Delta\theta_t)$	$\sigma(\Delta m_t) + (\sigma - 1)(\Delta\theta_t)$
$Z_{it} = E[l_{it}^2]$	$\varepsilon^2 V_m + \left(\varepsilon - \frac{\varepsilon}{\sigma}\right)^2 V_\theta$	$\sigma^2 V_m + (\sigma - 1)^2 V_\theta$

Note: See Table 1 note.

Equations (A13) and (A14) lead to expressions for firm i's second-period loss for each of the two possible lengths of other firms' contracts. Table 2 outlines the derivation. Once again, long contracts have negative externalities if and only if $\sigma > \varepsilon$. This is true even if monetary shocks do not exist (i.e., $V_m = 0$).

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Social Security and Individual Welfare: Precautionary Saving, Borrowing Constraints, and the Payroll Tax

By R. GLENN HUBBARD AND KENNETH L. JUDD*

This paper examines the impact of Social Security on national saving and individual welfare in the presence of realistic capital market imperfections — market failure in the private provision of annuities and restrictions on borrowing against anticipated future wages. The introduction of Social Security increases lifetime welfare and reduces national saving if borrowing restrictions are absent. However, the increase in individual welfare is reduced, and in some cases eliminated, when borrowing constraints are taken into consideration. The substantial difference suggests the importance of reexamining the proportional payroll tax finance of Social Security.

It has been recognized for some time in applied public economics that discussion of the impact of taxation and public programs on individual welfare (as well as on such aggregate measures of interest as the saving rate or the capital stock) requires an explicit analysis of agents' intertemporal budget constraints.[1] Since the pioneering paper by Martin Feldstein (1974), studies of the effects of Social Security on saving have considered behavior over the whole life cycle and not just in old age. Empirical work has tested the impact on consumption of the individual wealth transfers accompanying the introduction of a pay-as-you-go Social Security system.[2] Our concern here is with the impact of Social Security retirement annuities on national saving and individual welfare in the presence of realistic capital-market imperfections. In particular, we consider the relationship between two: (*i*) market failure in the private provision of annuities; and (*ii*) restrictions on borrowing against anticipated future wages.

The first has been examined in isolation. Andrew Abel (1985) and Hubbard (1987) have shown in the context of lifetime uncertainty that even an actuarially fair, fully funded Social Security system can generate increases in individual lifetime welfare. Hence, previous estimates of the impact of Social Security on consumption drawn solely from the consideration of the intergenerational wealth transfers at the introduction of the system may even be too small.

However, the specification of a *lifetime* budget constraint may be too narrow a de-

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[1] See, for example, the survey in Laurence Kotlikoff (1984).

[2] Feldstein's results have by no means gone unchallenged; see, for example, Robert Barro (1974, 1978), Dean Leimer and Selig Lesnoy (1982), and the reply to Leimer and Lesnoy in Feldstein (1982). Cross-sectional evidence has been supportive of the proposition that social security has reduced individual saving; see Feldstein and Anthony Pellechio (1979), Kotlikoff (1979b), Alan Blinder et al. (1981), Peter Diamond and Jerry Hausman (1984), Mervyn King and Louis Dicks-Mireaux (1982), and Hubbard (1986).

scription of restrictions on individuals' optimizing behavior in the presence of capital-market imperfections. Actual limitations on borrowing appear in upward-sloping interest rate schedules, collateral requirements, and quantity restrictions. It is clear on a qualitative level that the presence of liquidity constraints can reduce substantially the welfare gains from introducing Social Security annuities under payroll tax finance.

While the qualitative findings above are intuitive, the potentially important interaction of the capital-market imperfections requires a model with substantial intertemporal disaggregation. Our approach differs from Abel in that we employ life cycle simulation models to analyze capital-market imperfections. We do so for two reasons. First, given our interest in alternative public policies, it is important to obtain realistic quantitative estimates of the effects of Social Security. Second, knowledge of the length of periods during which borrowing restrictions bind is important; even a three-period analytical model would be inadequate for our purposes.

Recent advances in the examination of efficiency gains from dynamic tax reforms have used life cycle simulation models to isolate intragenerational and/or intergenerational effects.[3] The capital-market imperfections discussed above are typically missing in these models. First, most of these exercises have ignored insurance features of fiscal policies; social insurance programs affect agents' lifetime budget constraints to the extent that the private insurance markets (against uncertainty over length of life, job loss, catastrophic illness, etc.) are incomplete.[4] We focus on Social Security retirement annuities both because of their importance in the existing literature on tests of the life cycle model and because of the way in which they are financed.

Second, liquidity constraints are likely to be important. The extent to which agents can spread the benefits from participation in Social Security annuities over their lifetimes depends on the degree to which capital markets permit consumption smoothing when current resources are insufficient. More important, since Social Security is financed through a proportional payroll tax on current earnings, payroll taxes depress consumption dollar for dollar when liquidity constraints are binding. Including realistic limitations on borrowing introduces the possibility that increasing the provision of Social Security coverage (financed by the payroll tax) may leave individuals worse off in terms of utility gained from lifetime consumption, while at the same time increasing potential lifetime resources. In general, one expects that an optimal tax structure should reallocate this burden over an individual's lifetime.

We organize our analysis of the importance of capital-market imperfections (market failure in the private provision of annuities and borrowing restrictions) in describing the impact of Social Security on individual welfare and the capital stock as follows. In Section I, we investigate the relevance of borrowing restrictions and uncertainty over longevity for the size of the capital stock. In particular, we find that the stock of "precautionary saving" is quantitatively important, lending further support to the claim that the perfect-certainty version of the life cycle model cannot explain observed saving behavior.

In Section II, we take up the partial-equilibrium effects on individual consumption.

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[3] The focus of such analyses is generally on switching tax regimes, say from a proportional general income tax to a proportional consumption or wage tax. See for example Lawrence Summers (1981), Alan Auerbach and Kotlikoff (1983), Owen Evans (1983), Laurence Seidman (1984), and our paper (1986). Auerbach et al. (1983) and our paper (1986) have considered progressive taxation as well.

[4] It is possible to think of the "event-conditioned" transfer programs that comprise social insurance as relaxing constraints on individual consumption. For example, one of the primary goals of the Social Security retirement program is the maintenance of consumption in old age. When social insurance is viewed in the framework of precautionary saving (see also Daniel Hamermesh, 1982), its provision will in general affect lifetime consumption, and not just consumption during the periods in which payments are received.

As noted previously by Abel and Hubbard (1987), for any plausible set of assumptions about underlying parameters, social security generates a significant increase in lifetime consumption and welfare accompanied by a reduction in the capital stock if borrowing restrictions are absent. However, the increase in individual welfare is reduced, and in some cases eliminated, when borrowing restrictions are also taken into consideration. The substantial difference suggests the importance of reexamining the proportional payroll tax finance of Social Security.

In Section III, we extend the model to general equilibrium, with endogenous factor prices. Partial-equilibrium gains in lifetime welfare from participation in Social Security are offset in the long run by the interaction of higher steady-state interest rates and binding liquidity constraints. In contrast to the large welfare gains found in previous studies, we find that the steady-state welfare cost of Social Security under proportional payroll tax finance is in general substantial. Section IV illustrates the ability of alternative proposals for financing Social Security to alleviate the problem created by the interaction of borrowing constraints and the proportional payroll tax. Age-specific tax schemes can restore much of the potential gain from participating in Social Security annuities. Conclusions and directions for future research are discussed in Section V.

I. Lifetime Uncertainty, Borrowing Restrictions, and Individual Saving Behavior

Our emphasis in this paper is on the impact of Social Security annuities on national saving and individual welfare when there are imperfections in capital markets. We first consider the role of precautionary saving against lifetime uncertainty in the absence of an annuity market. Menahem Yaari's (1965) seminal paper showed that with an uncertain lifetime, intertemporal utility maximization can dictate saving against the possibility of living longer than expected. More recent applications to public pension schemes have appeared in James Davies (1981), Eytan Sheshinski and Yoram Weiss (1981), Abel, and Hubbard (1987). To the extent that precautionary saving is significant, modifying the basic life cycle model to include uncertainty may account for much of the failure of the model to explain wealth-age profiles (particularly among the elderly).[5]

Such precautionary saving is necessary because of market failure in the private provision of old-age annuities. This market failure is likely because of asymmetries of information between individuals and insurers, the classic adverse selection problem discussed by Michael Rothschild and Joseph Stiglitz (1976) and elaborated in the context of Social Security by Zvi Eckstein et al. (1985). Benjamin Friedman and Mark Warshawsky (1985) show that under plausible assumptions about risk aversion, annuity contracts actually offered in the market would not be purchased by optimizing individuals.[6]

Following Abel and Hubbard (1987), we do not explicitly model the reason for the absence of annuity markets. We assume that private annuities are not available. This is an appropriate assumption given our focus on the importance of the interaction of borrowing constraints and the Social Security system. By ruling out *all* annuities, we make the impact unrealistically large and bias the results in favor of Social Security. If we were to add reasonable features such as overpriced annuities, manipulative bequest motives (B. Douglas Bernheim et al., 1985), or altruistic bequest motives, the value of Social Security annuities would decline. The importance of liquidity constraints would not be affected, however, implying that our negative results would be more likely to hold.

We begin with the following life cycle model. Agents are assumed to be selfish, in that no bequests are desired. Individuals live for a maximum of T years, working only for the first R years; the retirement age of R is taken as exogenous, and labor is supplied inelastically. The probability of surviving through period t is $p(t)$ for each t. Our simulations begin at the beginning of individuals' working lives (assumed to be age 20). Retirement occurs at model age 45. The maximum model age to which one can survive is 90.

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[5] In addition, the contribution of "uncertainty saving" to the size of the capital stock could be part of the explanation for the finding by Kotlikoff and Summers (1981) that pure life cycle wealth is small relative to the stock of intergenerational transfers.

[6] To illustrate the unimportance of nonpension annuities in the United States, only about 1 percent of households surveyed in the 1962 *Survey of Financial Characteristics* held any annuities, with holdings of less than 0.1 percent of household net worth for those who did (see Dorothy Projector and Gertrude Weiss, 1966).

Following Yaari and Robert Barro and James Friedman (1977), we let utility be additively separable across periods, and let utility from consumption $U(c(t))$ be evaluated contingent on being alive at time t. That is, with no restrictions on borrowing, individuals choose c so as to maximize

$$(1) \qquad \int_0^T pU(c)e^{-\rho t}\,dt,$$

subject to

$$(2) \qquad \dot{A} = w + rA + B - c,$$

$$A(0) = 0, \quad A(T) \geq 0.$$

where c, ρ, and r represent consumption and the subjective discount rate and interest rate, respectively. The variable A represents the stock of accumulated assets. A dot over a variable denotes a time rate of change. The income stream w represents labor earnings; B includes resources from unplanned bequests from the previous generation.[7]

Assuming that the utility function is of the isoelastic form, we can rewrite (1) as

$$(3) \qquad \max_c \int_0^T \frac{1}{1-\beta^{-1}} pc^{1-\beta^{-1}} e^{-\rho t}\,dt,$$

where β is the intertemporal elasticity of substitution in consumption. Note that if $h(t)\,dt$ is the probability of death during $(t, t+dt)$ conditional on being alive at t, the hazard rate, the problem becomes

$$(4) \qquad \max_c \frac{1}{1-\beta^{-1}} \int_0^T e^{-\int_0^t(\rho+h(s))\,ds} c^{1-\beta^{-1}}\,dt,$$

subject to (2). Denoting the marginal utility of consumption conditional on being alive at t by $\lambda(t)$ and the sum of the rate of time preference and the hazard rate reflecting lifetime uncertainty at t by $\tilde{\rho}(t)$, the differential equations describing the time paths of consumption and asset accumulation (in the absence of borrowing restrictions) are given by

$$(5a) \qquad \dot{\lambda} = (\tilde{\rho} - r)\lambda,$$

$$(5b) \qquad \dot{A} = rA + w + B - C(\lambda),$$

where $C(\lambda)$ is defined by $U'(C(\lambda)) = \lambda$, expressing consumption as a function of the current marginal value of assets. Furthermore, we impose the boundary conditions $A(0) = A(T) = 0$.

Within the framework of the model described above, we can simulate the effect of lifetime uncertainty on the size of the capital stock. The total capital stock is aggregated up from age-specific individual asset stocks assuming a population growth rate of 1 percent per annum. Individuals in the certain-lifetime case are assumed to die at the average age of death in the population. Data on average survival probabilities are taken from J. F. Faber (1982). The individual age-earnings profile is taken from Davies.[8] The rate

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[7] The corresponding problem for the certain-lifetime case would be to maximize

$$\int_0^D U(c)e^{-\rho t}\,dt, \quad \text{subject to} \quad \dot{A} = w + rA - c,$$

where D is the expected date of death in $(0, T)$ in the uncertain-lifetime case.

[8] As in Davies (p. 572), the lifetime path of mean noninvestment income E is approximated from ages 20 to 65 by a fourth-order polynomial:

$$E(t) = 36{,}999.4 + 3520.22t - 101.878t^2$$
$$+ 1.34816t^3 - 0.00706233t^4.$$

An alternative earnings function would be the estimate by Finis Welch (1979) for full-time workers with a high-school education. We used that earnings profile as well; the simulation results differed little, and we do not report them here.

of time preference[9] ρ is assumed to equal 0.015 per annum.

There is some evidence on the value of β in the literature. Summers (1982) reports estimates of the intertemporal elasticities of about unity, using postwar data. Robert Hall (1985) concludes from a set of results that β is roughly zero. The results of the study by Irwin Friend and Marshall Blume (1975) of household portfolio allocation imply a β of no more than 0.5. Henry Farber's (1978) estimation of preferences of workers from collective bargaining agreements yielded results consistent with a β of about 0.3. Lars Peter Hansen and Kenneth Singleton (1983) found implied estimates of β of at least 0.5. In our simulation exercises, we use four values of β (1.10, 0.50, 0.25, and 0.20) along with three alternative values of r (0.02, 0.04, and 0.06).

Bequests are incorporated in the simulations as follows. Using information on $p(t)$ over the life cycle, a distribution of initial bequests can be generated. We consider intergenerational transfers from a generation of single "parents" to the next generation. Iteration proceeds for a given set of parameter values until an individual would transmit (in expected present value) the same bequest he or she receives. As our principal concern is with the first-order effects of lifetime uncertainty (and later Social Security) on the aggregate capital stock (and output and consumption), we do not discuss the impact of lifetime uncertainty or Social Security on the steady-state *distribution* of bequests (see, for example, Abel). Given our emphasis on life cycle patterns of consumption and savings, it is important that we do not aggregate intertemporally. We must allow the individual to live for several periods if we are to get a quantitative idea of the importance of liquidity constraints. We use a year as our unit of time; we shall see that a year is large enough that any substantially greater unit would involve too much aggregation. However, given this fineness in our intertemporal consumption patterns, it would be numerically intractable to calculate a steady-state rational expectations distribution of bequests. Therefore, all individuals are assumed to receive the weighted-average bequest regardless of their particular family mortality history. The implicit assumption is that individual bequests are taxed away by the government and redistributed lump sum to individuals. To model the observation that these bequests are most likely to occur when the recipient is in early middle age, such receipts are assumed to be obtained after twenty periods.[10]

An additional critique of standard life cycle models is that borrowing restrictions inhibit the ability of individuals to carry out their optimal age-consumption profile. One such limitation—and the one which we employ here—is a collateral restriction, so that net worth must be nonnegative at all times.[11]

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[9] Our results in the absence of borrowing restrictions are qualitatively robust to changes in $\rho(\rho < r)$. We chose a rate of 1.5 percent, which we believe to be in the lower range of results of previous studies, so as to avoid overemphasizing the effect of the liquidity constraint. That is, higher values of ρ would increase desired consumption in youth, magnifying the loss in welfare from the borrowing restriction.

[10] We bias the results against the possibility of significant effects of liquidity constraints by allowing bequests to be received as early as the twentieth period of life. The results reported are not very sensitive to changes in the timing of the receipt of the bequest after twenty periods.

[11] The source of borrowing restrictions can be important. Fumio Hayashi (1985b) and Toshiki Yotsuzuka (1986) have argued within the credit rationing model of Dwight Jaffee and Thomas Russell (1976) that neutrality prevails in the presence of liquidity constraints; that is, liquidity constraints adjust to changes in fiscal policy, restoring Ricardian equivalence. Bernheim (1987) shows that the Hayashi-Yotsuzuka results are sensitive to assumptions about the distribution of future tax liabilities. Hayashi and Yotsuzuka assume that taxes are fixed and lump sum. Bernheim considers the case in which taxes depend positively on earnings, and shows that Ricardian equivalence is in general violated. In our paper (1987), we emphasize the importance of capital-market imperfections such as those discussed here for analyses of short-run fiscal policy.

The form of borrowing restrictions we stress will be explcitable by public policy. In addition, the sort of collateral restrictions on loans to borrowers under asymmetric information discussed in Charles Calomiris and Hubbard (1986) would be consistent with this

That this restriction is most likely to be binding for the young implies that consumption will be shifted to later years in life for the representative individual, and the aggregate capital stock will be larger than it would have been if capital markets were not subject to this restriction.[12]

The imposition of a borrowing restriction of this form requires motivation. Here we rely on observed collateral requirements for borrowing in U.S. capital markets, that is, the restricted access to "consumption loans." Institutional motivations for the constraint include legal restrictions prohibiting the inclusion of human capital as an asset in bankruptcy proceedings or, also for our purposes, the assignment of future Social Security benefits. Allowing individuals to borrow some fixed "small" amount would increase significantly the numerical complexity of the problem, without qualitatively altering the results. Taken together, our assumptions about market failure in the private provision of annuities and borrowing restrictions should introduce no bias a priori, since the former magnifies the potential impact of Social Security on lifetime welfare, and the latter reduces it.

There is extensive empirical evidence on the importance of liquidity constraints (Fumio Hayashi, 1982, 1985a; Marjorie Flavin, 1984; Stephen Zeldes, 1985).[13] Using data from the Federal Reserve Board's 1983 "Survey of Consumer Finances," in our paper (1986), we show that a nontrivial fraction of U.S. households have holdings of financial assets that would be too small to insulate consumption from even moderate decreases in current earnings in the presence of restrictions on borrowing against future earnings. It is, of course, possible that liquidity-constrained consumers could borrow against net nonfinancial assets such as equity in homes. However, the tapping at any significant level of housing equity to finance consumption is a recent response to capital gains on housing. Moreover, information from the 1983 "Survey of Consumer Finances" shows that homeownership rates and home equity are low for the young (liquidity-constrained consumers in the model described below).[14]

When we impose the constraint that net worth must be nonnegative at all times, we substantially change the nature of the consumer's optimization problem. The budget constraint in (2) becomes

$$\dot{A} = w + rA + B - c; \; A(t) \geq 0, \quad \text{for all } t. \tag{2'}$$

The first-order conditions must be altered to take into account this state constraint (see Morton Kamien and Nancy Schwartz, 1982, for a discussion of such problems). The new

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analysis. Stephen Zeldes (1986) considers the case in which individuals are permitted to borrow against the "certain component" of future resources.

[12] The existence of borrowing restrictions in the form of collateral requirements does not, of course, necessarily imply that they will be binding. In our 1987 paper we discuss a case in which individuals begin their working lives with a common wage and experience a stochastic transition to a higher wage. Consumption is driven to its upper bound of current earnings when the prospective wage increase and its likelihood are high.

[13] Hayashi (1982) found that approximately 20 percent of all consumption in the United States could be accounted for by liquidity-constrained consumers. Additional evidence in support of liquidity constraints is provided by Hayashi's (1985) analysis of household data from the 1963 *Survey of Consumer Finances*, in which the relationship between consumption and income movements differs significantly between "high saving" and "low saving" families. Zeldes (1985) also reports that the sensitivity of consumption to changes in income depends on the level of household wealth. Ben Bernanke (1984) found no evidence against the permanent income hypothesis in his examination of individual expenditures on automobiles. Such loans, however, are self-collateralized. Flavin finds that the estimated marginal propensity to consume out of transitory income is explained almost entirely by proxies for liquidity constraints.

[14] We would suggest that our characterization of borrowing restrictions actually understates the importance of liquidity constraints in the real world. We only disallow net consumption loans. Real-world restrictions in addition to collateral (for example, minimum-income requirements for debt service) would strengthen our conclusions.

arbitrage equations become

$$(5a') \quad \dot{\lambda} = (\tilde{\rho} - r)\lambda, \qquad \text{if } A > 0 \quad \text{or} \quad \lambda > U'(w),$$

$$\lambda = U'(w), \qquad \text{if } A = 0 \quad \text{and} \quad \lambda \leq U'(w).$$

$$(5b') \quad \dot{A} = 0, \text{ if } A = 0, \quad \lambda \leq U'(w), \quad \text{and} \quad \int_0^t B(s)\, ds \text{ is continuous at } t,$$

$$\dot{A} = w + rA + B - C(\lambda), \quad \text{otherwise.}$$

If assets are positive or if wages exceed consumption, then equation (5a) still holds. Otherwise, consumption is limited to current earnings. This divides the consumer's problem into constrained and unconstrained periods of time. Equation (5b′) governs how these intervals meet. At the moment when the constraint that $A \geq 0$ becomes binding, it imposes the tangency relation between A and the constraint.[15] Intuitively, it just states that consumption must be continuous at the moment A becomes zero; concavity of $U(c)$ implies that one does not suddenly alter consumption when assets become zero.

The importance of capital-market imperfections for the capital stock is illustrated clearly in Table 1, which reports values for the aggregate savings ratio (K/Y) in the certain-lifetime and uncertain-lifetime cases, the value of average unplanned bequests as a fraction of lifetime earnings, and information on the incremental effects of liquidity constraints. As in Hubbard (1987), in the absence of borrowing restrictions, for all assumptions about the interest rate or the intertemporal elasticity of substitution in consumption, unplanned bequests accompanying lifetime uncertainty are significant relative to lifetime earnings, and capital-income ratios are substantially higher in the uncertain-lifetime case. Aggregate capital stocks implied by the life cycle model when lifetime is certain are small relative to those implied by the uncertain-lifetime case. The unrealistically low K/Y ratios in the certain-lifetime case lend further support to the finding in Kotlikoff and Summers (1981) that the basic life cycle model can explain only a small portion of the aggregate capital stock. The importance of precautionary saving could help to explain this discrepancy without relying on intentional bequest motives.

The second part of Table 1 points up the additional relevance of liquidity constraints for the size of the capital stock in the uncertain-lifetime case. In most cases, the constraint is binding for a significant interval;[16] the aggregate capital-income ratio is higher in the liquidity-constrained regime.

The potential welfare effects of liquidity constraints in this framework stem from the excess of desired consumption over earnings when young. These effects could be mitigated, of course, to the extent that individuals have a "humped" pattern of desired consumption as well—say, to finance the expenses of rearing and educating children in middle age (see, for example, Gilbert Ghez and Gary Becker, 1975). In our paper (1986), we show that the general results of the impact of liquidity constraints on saving and welfare are robust to the consideration of

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[15] We are, of course, considering liquidity constraints that occur in youth. We are not addressing random earnings per se (see for example our 1987 paper). Given our emphasis on the problem faced by individuals reacting to an upward-sloping lifetime earnings profile; the restriction is probably not serious. Since the marginal loss due to a tighter liquidity constraint is negligible if the constraint is light and greater when the constraint is tight, losses due to the liquidity constraint are convex in the tightness of the constraint. This indicates that the losses that arise are *underestimated* by examining an *average* earnings pattern, since the distribution of earnings patterns would include some with much tighter constraints as well as some with looser constraints.

[16] Walter Dolde (1978) documents the important influence of liquidity constraints on age-specific marginal propensities to consume. With no capital-market imperfections, the life cycle model predicts that marginal propensities to consume would increase monotonically with age. He shows that borrowing restrictions are significant in determining effective planning horizons for young (constrained) individuals.

TABLE 1—CAPITAL-MARKET IMPERFECTIONS AND SAVING BEHAVIOR

Role of Lifetime Uncertainty (No Borrowing Constraints)

	$(K/Y)_{CL}$ β				$(K/Y)_{UL}$ β				Expected Bequest as a Percentage of Lifetime Earnings β			
	1.10	0.50	0.25	0.20	1.10	0.50	0.25	0.20	1.10	0.50	0.25	0.20
$r = 0.02$	1.93	1.28	1.02	0.93	3.63	5.97	8.00	9.04	9.2	15.8	22.2	25.7
$r = 0.04$	5.29	2.56	1.34	0.93	7.45	6.69	6.86	7.10	19.1	20.6	23.4	25.1
$r = 0.06$	7.35	3.61	1.67	0.97	9.40	7.13	6.15	5.88	38.5	26.7	23.8	23.3

Liquidity Constraints and Lifetime Uncertainty

	Periods Constrained				$(K/Y)'_{UL}$			
	1.10	0.50	0.25	0.20	1.10	0.50	0.25	0.20
$r = 0.02$	9	8	7	7	4.86	7.11	9.01	9.99
$r = 0.04$	4	7	8	8	7.70	7.37	7.79	8.11
$r = 0.06$	2	5	8	9	9.42	7.49	6.94	6.86

Note: "CL" and "UL" refer to the certain-lifetime and uncertain-lifetime cases, respectively. $(K/Y)'_{UL}$ refers to the case where borrowing restrictions are imposed.

changes in family size over the life cycle (using the family weights presented in Walter Dolde). We modified the utility function in (1) to $\int_0^T n(t) pU(c) e^{-\rho t} dt$, where c now represents consumption per capita in the household and n represents the number of equivalent adults at time t. In fact, the liquidity constraints were shown to be more binding and last longer. For reasons of economy, we limit our presentation below to the nonfamily case.

II. The Impact of Social Security on Individual Welfare and the Capital Stock

In previous papers, Abel and Hubbard (1987) have shown that public provision of annuities through compulsory public pensions (Social Security) leads to partial equilibrium increases in individual welfare and decreases in national saving. In the context of our multiperiod life cycle model, a brief outline of such a Social Security system follows. Individuals are compelled to pay a payroll tax t_s on gross wages, from which the Social Security system is funded. During retirement they receive an annuity benefit S in each period t until death. The asset accumulation constraint (in the absence of borrowing restrictions) becomes

$$(6) \quad \dot{A} = rA + (1 - t_s) w + B + S - c; \qquad A(T) = 0.$$

If benefits are set according to a replacement rate of the terminal wage, then the economywide actuarially fair benefit S satisfies the condition that

$$(7) \quad S\int_R^T pe^{-rt}\,dt = t_s\int_0^R pwe^{-rt}\,dt.$$

In the absence of borrowing restrictions, the system generates an increase in the propensity to consume out of lifetime resources. This increase in lifetime consumption occurs even in a system which is actuarially fair and fully funded.

With respect to our focus on liquidity constraints, a potential welfare gain from the introduction of Social Security comes about because of increases in *preretirement* consumption made possible by the annuity provisions. With a nonnegativity constraint on net worth, however, the Social Security payroll tax depresses preretirement consumption as long as the constraint binds,

TABLE 2—SOCIAL SECURITY, THE CAPITAL STOCK, AND INDIVIDUAL WELFARE

	β	$r=0.02$	$r=0.04$	$r=0.06$
Percentage Change in K/Y	1.10	−48.2	−27.7	−17.0
		(−28.4)	(−24.5)	(−16.7)
	0.50	−58.1	−50.4	−32.0
		(−39.2)	(−39.0)	(−29.1)
	0.25	−58.9	−65.2	−36.5
		(−42.4)	(−47.8)	(−31.4)
	0.20	−57.9	−70.1	−39.3
		(−42.7)	(−51.4)	(−32.8)
Percentage Change in Bequests as a Fraction of Lifetime Earnings	1.10	−88.0	−80.1	−73.8
		(−82.1)	(−78.6)	(−73.6)
	0.50	−83.5	−94.2	−95.5
		(−77.1)	(−89.5)	(−93.6)
	0.25	−79.3	−98.3	−97.5
		(−72.9)	(−93.3)	(−95.8)
	0.20	−76.7	−98.8	−98.3
		(−70.2)	(−94.5)	(−96.6)
Δ Change in Lifetime Utility as Equivalent Percentage Change in Earnings: Δ	1.10	4.4	5.4	6.1
		(3.5)	(5.0)	(5.9)
	0.50	8.2	6.6	3.8
		(4.5)	(3.9)	(2.4)
	0.25	11.1	7.7	1.9
		(0.1)	(−1.8)	(−3.5)
	0.20	12.6	8.1	1.3
		(−4.3)	(−5.2)	(−5.6)
Number of Periods Constrained	1.10	13	5	2
	0.50	12	9	7
	0.25	11	12	11
	0.20	10	13	12

Notes: Numbers in parentheses represent the alternative result in the presence of liquidity constraints. Δ is expressed as a percentage of lifetime earnings. Δ and the percentage change in K/Y are measured with respect to the corresponding no-Social Security cases under lifetime uncertainty. In all cases, $t_s = 0.06$.

and increases consumption after the constraint ceases to bind. Hence the effect of an actuarially fair Social Security system is to increase desired consumption of the young, while decreasing actual consumption due to the interaction of the payroll tax and restrictions on borrowing.[17]

In Table 2, we simulate the partial-equilibrium impact on initial participants of the imposition of an actuarially fair Social Security system financed by a proportional payroll tax of 6 percent.[18] Percentage changes in the capital-output ratio and the average bequests as a fraction of lifetime earnings are reported, as is the change in lifetime welfare, Δ, expressed in terms of a percentage change in lifetime earnings. As before, simulation results are conducted over four values of β and three values of r. Results both in the absence of and in the presence of liquidity constraints; numbers referring to the constrained case are in parentheses. For the constrained case, the number of periods for which the constraint is binding is also reported.

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[17] The extent to which the provision of retirement annuities depresses preretirement saving depends in part on bequest motives. It is not obvious that planned bequests weaken our analysis. If an individual plans to leave a bequest to his (her) child, his desired lifetime consumption is reduced. However, if he himself receives a bequest from a parent in some middle or late period, that bequest could be used to finance a bequest to his child. As long as the bequest he receives from his parent is obtained after his constrained periods and he plans on leaving a bequest of comparable size to his child, his consumption path is financed by his earnings; bequests received and given are not important for consumption decisions.

[18] We chose a moderate payroll tax rate between the rate assessed at the beginning of the system and the much higher rate in place now. The results are not qualitatively sensitive to the choice of the payroll tax rate. When the wealth nonnegativity constraint is introduced in the next section, welfare losses from the interaction of the borrowing constraint and the payroll tax increase with the tax rate in a convex fashion.

In the absence of borrowing restrictions, the system generates very large initial declines in the capital stock. Given the dramatic reduction in the size of accidental bequests shown in the table, this is not surprising.[19] Potential welfare gains to initial participants from introducing an actuarially fair Social Security system are significant for plausible parameter values.

Realistic borrowing restrictions mitigate these large welfare gains. Because the system is financed by a proportional payroll tax on earnings, forced saving occurs in youth. The last set of rows in Table 2 illustrates the importance of the restriction on borrowing against future net earnings even under a moderate Social Security tax, with consumption constrained to be no more than current resources for at least ten periods at an interest rate of 0.02. For moderate measures of the intertemporal elasticity of substitution in consumption ($\beta = 0.50$), the gains in lifetime welfare reported are substantially smaller than the potential gains in the absence of borrowing restrictions. When $\beta = 0.25$, the gains are trivial when the interest rate is 0.02, and at interest rates of 0.04 or 0.06, the operation of the Social Security system actually leads to a *loss* in lifetime welfare, as it does for all cases when $\beta = 0.20$. These results suggest the importance of both uncertainty and the method used to finance the system in evaluating the impact of Social Security on individual saving behavior. Before returning to the issue of financing the system in Section IV, we take up in Section III the problem of considering these effects when factor prices are endogenous.

III. General Equilibrium, Steady-State Impacts of Social Security on Individual Welfare, and the Capital Stock

To examine seriously the welfare effects of Social Security under different assumptions about capital market imperfections, we must analyze the new steady state after the system is introduced.[20] Changes in the steady-state capital stock will affect the level of output and consumption per head, and hence the lifetime utility of a representative agent. Members of the first generation in the system benefit both from the accidental bequest from the uninsured previous generation and from the gains from participating in Social Security annuities.[21] The reduced value of accidental bequests permits smaller gains in consumption for succeeding generations. Hence, to consider the potential welfare gains from compulsory pensions, the tradeoff between the benefits to early participants and the costs of a lower capital stock to subsequent generations must be examined.

The partial-equilibrium effects of Social Security on individual saving will be dampened in a general equilibrium analysis of the impact on aggregate capital formation, once factor-price changes are taken into account. Such considerations have been examined in certain-lifetime models. For example, Kotlikoff (1979a) used a life cycle model with certain longevity and a Cobb-Douglas production technology to consider the impact of a pay-as-you-go Social Security system on the capital stock. For plausible parameter values, he found that the positive lifetime wealth increment traceable to Social Security caused a 20 percent decrease in the steady-state capital stock. While this effect is certainly substantial, it is roughly half of his calculated partial-equilibrium effect which is directly related to the excess of the present value of benefits over the present value of contribution.

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[19] For example, Kotlikoff and Avia Spivak (1981) report very large welfare gains from the introduction of a perfect annuity market.

[20] An analysis of the transition between steady states would be desirable, but problematic. Overlapping generations models such as the class discussed here can have a continuum of transition paths (see the discussion in Timothy Kehoe and David Levine, 1985), implying that the results of comparative-dynamic experiments are indeterminate. Some attempts have been made at transition analysis (in particular, Auerbach et al.), but their results must be regarded as tentative; see the more detailed discussion of this issue in our paper (1986).

[21] Of course, the extent to which this benefit can be realized depends on the extent to which borrowing constraints are binding when young. As shown before, liquidity constraints can sharply reduce gains in consumption due to Social Security even in a partial-equilibrium model.

To examine the impact of savings against lifetime uncertainty on aggregate saving, we assume that output is produced according to a Cobb-Douglas production function in capital and labor, with a capital share equal to α. Factor markets are assumed to be competitive, so that capital and labor are paid their marginal products. That is, the interest rate (r) and wage rate (w) are such that

$$r = \alpha k^{\alpha-1}, \tag{8}$$

$$w = (1-\alpha)k^{\alpha}, \tag{9}$$

where k represents the capital-labor ratio.

Within this framework, the steady state can be solved for as follows. A guess is made for k. Solutions for r and w are then generated from the marginal productivity conditions to produce individual consumption and wealth profiles. The resulting aggregate consumption and capital stock per capita are compared with the initial guess, and iteration proceeds until convergence is reached.

As in the partial-equilibrium case, a second calculation of the value of the steady-state bequest must also be made. Within the routine described above, each parameterization of r and w generates a different expected bequest, which is then transferred to the child.

Our analysis of the general equilibrium impact of Social Security on the capital stock and individual welfare proceeds in four exercises, the results of which are reported in Table 3. In all cases, α, capital's share, is assumed to equal 0.30, and $\rho = 0.015$. First, we compute the initial steady-state values of the interest rate, capital-output ratio, average bequest relative to lifetime earnings, and lifetime welfare in the absence of Social Security for each of the four values of β. Those results are reported in panel A of Table 3. As expected, lower levels of the intertemporal elasticity of substitution in consumption are associated with higher average bequests and capital stocks, and hence lower steady-state interest rates.

TABLE 3—GENERAL EQUILIBRIUM EFFECTS OF SOCIAL SECURITY ON THE CAPITAL STOCK AND INDIVIDUAL WELFARE

	r	K/Y	b	Δ
	A. Initial Steady State			
$\beta = 1.10$	0.035	8.51	21.1	–
$\beta = 0.50$	0.031	9.62	26.9	–
$\beta = 0.25$	0.024	12.48	35.8	–
$\beta = 0.20$	0.020	14.66	42.1	–
	B. Imposition of Social Security (Fixed r, Fixed Bequest)			
$\beta = 1.10$	0.035	6.86	6.9	+3.8
$\beta = 0.50$	0.031	7.79	10.2	+0.3
$\beta = 0.25$	0.024	10.18	17.8	−4.5
$\beta = 0.20$	0.020	12.23	24.3	−4.9
	C. Imposition of Social Security (Fixed r, Bequest Adjusts)			
$\beta = 1.10$	0.035	5.51	4.1	−8.2
$\beta = 0.50$	0.031	4.78	3.8	−9.0
$\beta = 0.25$	0.024	5.90	7.6	−6.9
$\beta = 0.20$	0.020	7.34	11.7	−7.4
	D. Imposition of Social Security (New Steady State)			
$\beta = 1.10$	0.044	6.74	3.5	−14.7
$\beta = 0.50$	0.058	5.22	1.8	−27.9
$\beta = 0.25$	0.061	4.87	1.1	−37.2
$\beta = 0.20$	0.063	4.78	1.0	−41.8

Note: In all cases, $\rho = 0.015$, and $t_s = 0.06$. Δ is measured with respect to changes from the initial no-Social Security steady state. b represents the average bequest as a percentage of lifetime earnings.

To obtain the results in panel B of Table 3, we introduce actuarially fair Social Security annuities financed by a proportional payroll tax of 6 percent, holding the interest rate and initial bequest constant at the levels from the original no-Social Security steady state. These results represent impacts on the initial generation to participate in Social Security. As the third column shows, average bequests are reduced substantially, as the initial generation to participate in Social Security obtains the dual benefits of a high initial bequest and access to Social Security annuities. Partial-equilibrium welfare gains are recorded for the cases where $\beta = 0.50$ or $\beta = 1.10$. The added burden of payroll contributions to Social Security during youth causes welfare losses for the two lower values of β ($\beta = 0.25$, $\beta = 0.20$). Moving to panel C, the interest rate is still fixed at its initial steady-state level, but the steady-state bequest available in future generations declines to its new steady-state level. As a result, capital supply and lifetime welfare continue to decline relative to their counterparts in the no-Social Security steady state.

Finally, in panel D of Table 3, the new steady state in the presence of Social Security is computed. As expected from the substantial reduction in bequests and the capital stock, steady-state interest rates increase considerably. These results are important for our analysis of the effects of Social Security annuities in the presence of capital-market imperfections. The interaction of the higher interest rates and lower earnings with the contribution of payroll taxes to binding liquidity constraints leads to significant reductions in lifetime welfare. For example, in the $\beta = 1.10$ case, a 3.8 percent partial-equilibrium increase in lifetime welfare becomes a 14.7 percent reduction in the new steady state.

We do not mean to imply that these calculations describe the historical impact of Social Security on the capital stock in the United States. Participants in Social Security have, for example, obtained returns much greater than the actuarially fair return. The assumption of complete market failure in the private provision of annuities leads to a large effect on desired lifetime consumption from the introduction of social security annuities. While the annuity market in the United States is very imperfect, it is not nonexistent; Kotlikoff et al. (1987) point to family risk-sharing arrangements, and Hubbard (1987) identifies the importance of private pensions as annuity substitutes. However, these qualifications only amplify our points about the limited welfare gains from introducing Social Security given payroll tax finance and liquidity constraints. In the next section, we illustrate the importance of liquidity constraints for these impacts by examining an alternative system of tax finance.

IV. "Progressive Taxation" and the Welfare Gains from Social Security

To examine the influence of the method of financing Social Security on its impact on the capital stock and individual welfare, we now remove the assumption of proportional payroll tax finance. In its place we institute a progressive Social Security tax in which the first fifteen working periods are exempt from payroll taxation. To preserve comparability with our previous results, the retirement benefit is kept the same as under the proportional tax case where $t_s = 0.06$. A new, higher flat tax rate is instituted in the sixteenth period to maintain the average actuarial fairness of the system. The use of an exemption (effectively, an "earned income credit") alleviates the added contribution to the Social Security payroll tax to liquidity constraints on consumption. By shifting the burden of the tax intertemporally through higher taxes later in life, the same present value of contribution can be collected with an increase in individual lifetime utility from consumption. Cross sectionally, the use of the exemption corresponds to progressive taxation.

In general, the shape of the optimal lifetime payroll tax schedule should reflect a tradeoff between the distortions in labor supply with distortions in individual consumption. Of course, the model presented here assumes that labor is supplied inelastically in all periods. A negative labor supply response to the higher payroll tax would necessitate still higher taxes later in life.[22] We considered the relative desirability of proportional and progressive general income taxation with liquidity constraints and elastic labor supply in our 1986 paper. Even with substantial labor supply elasticity, the

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[22] For a discussion of the impact of Social Security on preretirement labor supply, see Richard Burkhauser and John Turner (1978).

TABLE 4—"PROGRESSIVE" TAXATION AND SOCIAL SECURITY: REASSESSING THE IMPACT ON INDIVIDUAL WELFARE

	Δ			
	$\beta = 1.10$	$\beta = 0.50$	$\beta = 0.25$	$\beta = 0.20$
$r = 0.02$	4.2	6.3	4.3	1.4
	(3.5)	(4.5)	(0.1)	(−4.3)
$r = 0.04$	5.1	5.3	2.7	0.6
	(5.0)	(3.9)	(−1.8)	(−5.2)
$r = 0.06$	6.0	4.0	0.8	0.2
	(5.9)	(2.4)	(−3.5)	(−5.6)

Notes: Δ is measured with respect to the corresponding no-Social Security cases under lifetime uncertainty, with borrowing restrictions imposed. Individuals are exempt from Social Security taxation for the first fifteen working periods. Numbers in parentheses are the welfare gains from imposing a social security system of identical size financed by a proportional payroll tax.

effects of liquidity constraints were strong enough that intertemporal shifts in the burden of taxation over the lifetime were welfare improving.

Partial-equilibrium results for the impact of Social Security on lifetime welfare (comparable to Table 3) are presented in Table 4. Simulations are run over four values of β and three values of r, as before. The top entry in each cell represents the gain in lifetime welfare (expressed as an equivalent percentage of lifetime earnings) from participating in the "progressive-tax-financed" Social Security system. The numbers in parentheses below are the corresponding gains from the "proportional-tax-financed" system in Table 4. This change in the structure of payroll tax finance has important implications for the magnitude of the welfare gains made possible by Social Security. Gains are positive in all cases in Table 3, and for many parameterizations are substantially higher than in the proportional-tax-financed case. That delaying the timing of the tax collections can restore much of the potential gain from participating in Social Security puts in a new light claims based on "perfect-markets" models that large welfare costs necessarily accompany progressive taxation.

We can use this framework to consider the relative burden of the payroll tax on different groups. Suppose for example that individuals differ in their holdings of initial assets.[23] Individuals with significant levels of assets do not face reductions in initial consumption because of the payroll tax, and they obtain full benefits from participating in the Social Security annuity system. The payroll tax will exacerbate constraints on initial consumption for low-wealth individuals, whose ability to consume against prospective gains from participating in Social Security annuities is limited. Viewed in this way, the proportional payroll tax is more regressive than more standard models would suggest.

V. Conclusions

One of the original goals of the Social Security old-age benefit program was the maintenance of consumption in retirement. Over the past decade, however, many theoretical and empirical studies have focused on the impact on preretirement consumption of the provision of Social Security annuities. The consideration of realistic capital-market imperfections is important here. For example, with uncertainty over longevity and imperfections in private annuity markets, the introduction of even an actuarially fair Social Security system can generate a substantial increase in lifetime consumption and welfare. When borrowing against future resources is limited, however, the use of proportional payroll tax finance for Social Security increases the incidence of liquidity constraints on the consumption of individuals whose current resources are low relative to their future resources.

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[23] In our 1987 paper, we derive expressions for the marginal propensity to consume out of assets in a finite-horizon model with uncertain lifetime and earnings under various assumptions about initial assets.

Using simulation models under various assumptions about individual preferences and technology, we analyze the impact of precautionary saving against lifetime uncertainty and borrowing restrictions on individual welfare and the capital stock in the presence and absence of Social Security annuities. Our principal conclusions are two. First, while the introduction of an actuarially fair Social Security system leads to a significant increase in lifetime consumption and welfare, accompanied by a reduction in the capital stock, the gain is reduced—and in some cases eliminated—in the presence of realistic restrictions on borrowing. Extending the model to general equilibrium, we find that the partial-equilibrium gains in lifetime welfare from participation in Social Security are offset by the interaction of higher steady-state interest rates and binding liquidity constraints. Indeed, the steady-state welfare cost of Social Security under proportional payroll tax finance can be substantial. Second, replacing the proportional payroll tax with a progressive tax (essentially a linear tax with an exemption), we show that age-specific tax schemes can restore much of the potential gain from participating in Social Security. By modeling labor-supply responses to tax-induced changes in the net wage, more formal methods of optimal taxation can be applied (see, for example, our 1986 paper).

While we focus on the Social Security system, our approach can be extended to examinations of other fiscal policies within life cycle models. Social Security provides an appropriate starting point for analysis, since realizing the large potential welfare gains from the insurance features of the system depends importantly on agents' ability to smooth consumption over the life cycle. An obvious application of the emphasis on precautionary saving is to types of uncertainty other than that over longevity, and appropriate social insurance programs.

More generally, a broader class of issues arises with respect to analyzing fiscal policies in imperfect capital markets. To the extent that borrowing constraints play an important role, conclusions about the welfare effects of such policy reforms as altering the progressivity of the income tax, changing the tax base from income to consumption, or lowering taxes on capital income while raising labor-income taxes will have to be reexamined (as in our 1986 paper). Second, the importance of debates over "finite horizons" for analyses of fiscal policy may have been overstated. In our 1987 paper, we argued that assumptions about capital-market imperfections were quantitatively more important than distinctions between finite and infinite horizons in assessing the neutrality of *short-run* fiscal policy changes. Here, focusing on *long-run* intergenerational fiscal policy, the same is again true. Previous research emphasizing finite horizons relied on the actuarial unfairness of the Social Security system. In the absence of borrowing restrictions, such effects are, however, likely to be small relative to effects traceable to imperfect private insurance markets. With borrowing restrictions, those results are weakened substantially.

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Monopolistic Competition and the Effects of Aggregate Demand

By OLIVIER JEAN BLANCHARD AND NOBUHIRO KIYOTAKI*

How important is monopolistic competition to an understanding of the effects of aggregate demand on output? We ask the question at three levels. Can monopolistic competition, by itself, explain why aggregate demand movements affect output? Can it, together with other imperfections, generate effects of aggregate demand in a way that perfect competition cannot? If so, can it give an accurate account of the response of the economy to aggregate demand movements? The answers are no, yes, and yes.

Monopolistic competition provides a convenient conceptual framework in which to think about price decisions, and appears to describe many markets more accurately than perfect competition. But, how important is monopolistic competition for macroeconomics? In particular, how important is monopolistic competition to an understanding of the effects of aggregate demand on economic activity? This is the question we analyze in this paper.

One can ask the question at three levels. First, using perfect competition as a benchmark, can monopolistic competition, by itself, explain why aggregate demand movements affect output? Second, can monopolistic competition, together with some other imperfection, generate effects of aggregate demand in a way that perfect competition cannot? Third, taking as given that aggregate demand movements affect output, can monopolistic competition give a more accurate account of the response of the economy to aggregate demand shocks? The paper analyzes these three questions in turn.

Section I builds a simple general equilibrium model, with goods, labor, and money, and monopolistic competition in both the goods and the labor markets. It then characterizes the equilibrium. Section II characterizes the inefficiency associated with monopolistic competition and shows that it is associated with an aggregate demand externality. It shows that this externality cannot, however, explain why pure aggregate demand movements affect output. In particular, it cannot explain why changes in nominal money have real effects. The answer to the first question is therefore negative. Section III studies the effects of "menu costs" when combined with monopolistic competition. It shows that small (second-order) costs of changing prices may lead to large (first-order) changes in output and welfare in response to changes in nominal money. It shows the close relation between this result and the aggregate demand externality identified in Section II. The answer to the second question is therefore positive. Section IV takes as given that prices and wages do not adjust to movements in nominal money, and draws the implications of monopolistic competition with fixed costs for the behavior of output, productivity, profits, and entry by new firms in response to fluctuations in aggregate demand. These implications fit the facts and our answer to the third question is positive. We conclude by discussing how the implications for entry may in turn help explain the lack of adjustment of prices and wages.

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Our paper is closely related to three recent strands of research, on general equilibrium implications of monopolistic competition (Oliver Hart, 1982, and Martin Weitzman, 1982, in particular), on "menu costs" or "near rationality" (N. Gregory Mankiw, 1985, George Akerlof and Janet Yellen, 1985a, b) and on "coordination failures" (Russell Cooper and Andrew John, 1985, in particular). We shall point out specific relations as we proceed. Our intent is in part to show their relation and macroeconomic implications in the specific context of monopolistic competition.

I. A Model of Monopolistic Competition

In developing a model of monopolistic competition, we make four main choices. Because we want to focus on both wage and price decisions, we construct a model with both households and firms, with separate labor and goods markets. Both labor and goods markets are monopolistically competitive. The assumption of monopolistic competition in both sets of markets is made for symmetry and transparence rather than for realism. For some purposes, however, the model that has both wage and price setters is not the simplest, and we also occasionally focus on a special case, that can be thought of as an economy of household-firms, each producing a differentiated good; in that case, there is only one set of price setters and the analysis is simplified.

The second choice follows from the need to avoid Say's Law, or the result that the supply of goods produced by the monopolistically competitive firms automatically generates its own demand. To avoid this, agents must have the choice between these goods and something else. In the standard macroeconomic model, the choice is between consumption and savings. In other models of monopolistic competition, the choice is between produced goods and a nonproduced good (Hart, for example). Here, we shall assume that the choice is between buying goods and holding money. This is most simply and crudely achieved by having real money balances in the utility function. Thus, money plays the role of the nonproduced good and provides services.[1,2] Money is also the numeraire, so that firms and workers quote prices and wages in terms of money; this plays no role in this and the next section, but will become important in Section III.

The third choice is to make assumptions about utility and technology that lead to demand and pricing relations, which are as close to traditional ones as possible, so as to allow an easy comparison with standard macroeconomic models. In effect, we follow Avinash Dixit and Joseph Stiglitz (1977) in adopting constant elasticity of substitution specifications in utility and production.[3] These specifications lead to log-linear demand and pricing functions, and rule out potentially important nonlinearities as well as a potential source of multiple equilibria.[4]

Finally, the model is static and we leave the potentially important dynamic implications of monopolistic competition to future work.[5]

A. *The Model*

The economy is composed of m firms, each producing a specific good that is an imperfect substitute for the other goods, and n consumer-workers, households for short,

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[1] A Clower Constraint would lead to similar results. Developing an explicitly intertemporal model to justify why money is positively valued did not seem worth the additional complexity in this context.

[2] There are, however, differences between money and a standard nonproduced good that arise from the fact that real, not nominal, money balances enter utility; we shall point out these differences as we go along.

[3] The main alternative to product diversification à la Dixit-Stiglitz, is geographic dispersion. This is the approach followed by Weitzman.

[4] Our assumptions imply a constant elasticity of demand, and a constant markup of price over marginal cost. Some recent work on the implications of imperfect competition for macroeconomics has precisely focused on why markups may not be constant, either because of changes in elasticity of demand, or of changes in the degree of collusion between firms. See, for example, Stiglitz (1984), Julio Rotemberg and Garth Saloner (1986), and Mark Bils (1985).

[5] Kiyotaki (1985b) studies the implications of monopolistic competition for investment and the likelihood of multiple equilibria.

each of them selling a type of labor which is an imperfect substitute for the other types. As a result, each firm and each worker has some monopoly power.[6] We now describe the problem faced by each firm and each household.

Firms are indexed by i, $i=1,\dots,m$. Each firm i has the following technology:

$$(1)\quad Y_i=\left(\sum_{j=1}^{n} N_{ij}^{(\sigma-1)/\sigma}\right)^{(\sigma/(\sigma-1))(1/\alpha)},$$

where Y_i denotes the output of firm i. N_{ij} denotes the quantity of labor of type j used in the production of output i. There are n different types of labor, indexed j, $j=1,\dots,n$. The production function is a CES production function, with all inputs entering symmetrically. Introducing fixed costs would not affect the analysis of the first three sections where we assume the number of firms to be fixed; we shall, for simplicity, introduce them only in the last section where they matter.

The two parameters characterizing the technology are α and σ. The parameter σ is the elasticity of substitution of inputs in production. The parameter α is the inverse of the degree of returns to scale; $\alpha-1$ is the elasticity of marginal cost with respect to output, elasticity of marginal cost for short in what follows. To guarantee the existence of an equilibrium, we restrict σ to be strictly greater than unity, and α to be equal to or greater than unity.

The firm maximizes profits. Nominal profits for firm i are given by

$$(2)\quad V_i=P_iY_i-\sum_{j=1}^{n} W_jN_{ij},$$

where P_i denotes the nominal output price of firm i, and W_j denotes the nominal wage associated with labor type j. The firm maximizes (2) subject to the production function (1). It takes as given nominal wages and the prices of the other outputs. It also faces a downward-sloping demand schedule for its product, that will be derived below as a result of utility maximization by households. We assume that the number of firms is large enough that taking other prices as given is equivalent to taking the price level as given.

Households are indexed by j, $j=1,\dots,n$. Household j supplies labor of type j. It derives utility from leisure, consumption, and real money balances. Its utility function is given by

$$(3)\quad U_j=\left(m^{1/(1-\theta)}C_j\right)^{\gamma}\left(M_j'/P\right)^{1-\gamma}-N_j^{\beta},$$

$$\text{where}\quad C_j=\left(\sum_{i=1}^{m} C_{ij}^{(\theta-1)/\theta}\right)^{(\theta/(\theta-1))}$$

$$\text{and}\quad P=\left(\frac{1}{m}\sum_{i=1}^{m} P_i^{1-\theta}\right)^{1/(1-\theta)}.$$

The first term in utility is a consumption index (basket) that gives the effect of the consumption of goods on utility. C_{ij} denotes the consumption of good i by household j, and C_j is a CES function of the C_{ij}s. All types of consumption goods enter utility symmetrically. The parameter θ is the elasticity of substitution between goods in utility. To guarantee existence of an equilibrium, θ is restricted to be greater than unity. The constant in front of C_j, that depends on θ and on the number of products m, is a convenient normalization with the implication that an increase in the number of products does not affect marginal utility after optimization.

The second term gives the effect of real money balances on utility. γ is a parameter between zero and one. Nominal money balances are deflated by the nominal price index associated with C_j. We shall refer to P as the price level.

The third term in utility gives the disutility from work, N_j is the amount of labor supplied by household j. The term $\beta-1$ is the elasticity of marginal disutility of labor; β is assumed to be equal to or greater than unity.[7,8]

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[6] Since in equilibrium each labor supplier sells some of his (her) labor to all firms, it is more appropriate to think of labor suppliers as craft unions, or syndicates as in Hart, rather than individual workers. However, since we want to analyze labor supply and consumption decisions simultaneously, we shall continue to refer to labor suppliers as "consumer-workers" or "households."

Households maximize utility subject to a budget constraint. Each household takes prices and other wages as given. Again we assume that n is large enough that taking other wages as given is equivalent to taking the nominal wage level as given. It also faces a downward-sloping demand schedule for its type of labor, which will be derived as the result of profit maximization by firms. The budget constraint is given by

$$(4)\quad \sum_{i=1}^{m} P_i C_{ij} + M_j' = W_j N_j + M_j + \sum_{i=1}^{m} V_{ij},$$

where M_j denotes the initial endowment of money, and V_{ij} is the share of profits of firm i going to household j.

B. *The Equilibrium*

The derivation of the equilibrium is given in the Appendix. The equilibrium can be characterized by *a relation between real money balances and aggregate demand, a pair of demand functions* for goods and labor and by *a pair of price and wage rules.*

The relation between real money balances and real aggregate consumption expenditures, which we shall call aggregate demand for short, is given by

$$(5)\qquad Y = K(M/P)$$

where

$$(6)\qquad Y \equiv \left(\sum_{j=1}^{n} \sum_{i=1}^{m} P_i C_{ij} \right) \Big/ P$$

and

$$P \equiv \left((1/m) \sum_{i=1}^{m} P_i^{1-\theta} \right)^{1/(1-\theta)}.$$

The demand functions for goods and labor are given by

$$(7)\quad Y_i = \sum_{j=1}^{n} C_{ij} = K_c Y (P_i/P)^{-\theta} \qquad i = 1, \dots, m$$

$$(8)\quad N_j = \sum_{i=1}^{m} N_{ij} = K_n Y^{\alpha} (W_j/W)^{-\sigma} \qquad j = 1, \dots, n$$

where the wage index W is given by

$$(9)\qquad W = \left(\frac{1}{n} \sum_{j=1}^{n} W_j^{1-\sigma} \right)^{1/(1-\sigma)}.$$

The price and wage rules are given by

$$(10)\quad (P_i/P) = \left[(\theta/(\theta-1)) K_p \times (W/P) Y^{\alpha-1} \right]^{1/(1+\theta(\alpha-1))} \qquad i = 1, \dots, m$$

$$(11)\quad (W_j/W) = \left[(\sigma/(\sigma-1)) K_w \times (P/W) Y^{\alpha(\beta-1)} \right]^{1/(1+\sigma(\beta-1))} \qquad j = 1, \dots, n$$

The letters K, K_c, K_n, K_p, and K_w are constants that depend on the parameters of the technology and the utility function as well as the number of firms and households.

We interpret these equations, starting with the relation between real money balances and aggregate demand. First-order conditions for households imply that desired real money balances are proportional to consumption expenditures. Aggregating over households and using the fact that, in equilibrium, desired money is equal to actual money gives equation (5).

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[7] The assumption that utility is homogeneous of degree one in consumption and real money balances, as well as additively separable in consumption and real money balances on the one hand, and leisure, on the other, eliminates income effects on labor supply. Under these assumptions, competitive labor supply would just be a function of the real wage, using the price index defined in the text. It also implies that utility is linear in income and this facilitates welfare evaluations.

[8] For reasons that will be clear below, we exclude the case where both α and β are equal to unity.

The demand for each type of good relative to aggregate demand is a function of the ratio of its nominal price to the nominal price index, the price level, with elasticity $(-\theta)$. The demand for labor by firms is a derived demand for labor; it depends on the demand for goods. The demand for each type of labor is a function of the ratio of its nominal wage to the nominal wage index, with elasticity $(-\sigma)$.

We now consider the *price rule*. Given the price level, each firm is a monopolist and decides about its real (or relative) price P_i/P. An increase in the real wage (W/P) shifts the marginal cost curve upward, leading to an increase in the relative price. An increase in aggregate demand shifts the demand curve for each product upward; if the firm operates under decreasing returns, the marginal cost curve is upward sloping and the relative price increases. Under constant returns, the shift in demand has no effect on the relative price.

We finally consider the *wage rule*, equation (11). We can think of households as solving their utility-maximization problem in two steps. They first solve for the allocation of their wealth, including labor income, between consumption of the different products and real money balances. After this step, the assumption that utility is linearly homogeneous in consumption and real money balances implies that utility is linear in wealth, thus linear in labor income. The next step is to solve for the level of labor supply and the nominal wage. Given that utility is linear in labor income, we can think of households as monopolists maximizing the surplus from supplying labor. Formally, if μ denotes the constant marginal utility of real wealth, households solve in the second step:

$$\max \mu(W_j/P)N_j - N_j^{\beta};$$

$$N_j = K_n Y^{\alpha}(W_j/W)^{-\sigma}.$$

The real wage relevant for worker j is W_j/P, which we can write as the product $(W_j/W)(W/P)$. The demand for labor of type j is a function of the relative wage (W_j/W) as well as of aggregate demand.

An increase in the aggregate real wage (W/P) leads household j to increase its labor supply, thus to decrease its relative wage (W_j/W). An increase in aggregate demand leads, if β is strictly greater than unity, to an increase in the relative wage. If β is equal to unity, if the marginal disutility of labor is constant, workers supply more labor at the same relative wage, in response to an increase in aggregate demand.

C. *Symmetric Equilibrium*

Equilibrium and symmetry, both across firms and across households, implies that all relative prices and all relative wages must be equal to unity. Thus, using $P_i = P$ for all i and $W_j = W$ for all j, and substituting in equations (10) and (11) gives

$$(12)\quad (P/W) = (\theta/(\theta-1))K_p Y^{\alpha-1};$$

$$(13)\quad (W/P) = (\sigma/(\sigma-1))K_w Y^{\alpha(\beta-1)}.$$

Equation (12), obtained from the individual price rules and the requirement that all prices be the same, gives the price wage ratio (P/W) as a function of output. If firms operate under strictly decreasing returns, the price wage ratio is an increasing function of the level of output. Equivalently, the real wage (W/P) consistent with firms' behavior is a decreasing function of output. We shall refer to equation (12) as the "aggregate price rule."

Equation (13), obtained from the individual wage rules and the requirement that all wages be the same, gives the real wage (W/P) as a function of output. If β is strictly greater than unity, that is, if workers have increasing marginal disutility of work, an increase in output, that leads to an increase in the derived demand for labor, requires an increase in the real wage. The real wage consistent with households' behavior is an increasing function of output. We shall refer to equation (13) as the "aggregate wage rule."

Equations (12) and (13) give (W/P) and Y. The value of (M/P) follows from (5). The equilibrium is characterized graphically in Figure 1. As (12) and (13) are log-linear, we measure $\log(W/P)$ on the vertical axis

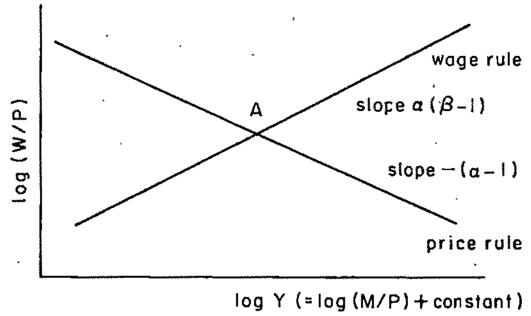

FIGURE 1. THE MONOPOLISTICALLY COMPETITIVE EQUILIBRIUM

and log(Y) (or log(M/P) as the two are linearly related) on the horizontal axis. If α and β are both strictly greater than unity, the aggregate wage rule is upward sloping, while the aggregate price rule is downward sloping. The equilibrium determines the real wage and output. Given output, we obtain the equilibrium level of real money balances and given nominal money, we finally obtain the price level.

Figure 1 looks like the characterization of equilibrium under perfect competition, with an upward-sloping labor supply curve and a downward-sloping labor demand. What is, therefore, the effect of monopolistic competition? Before turning to that issue, we briefly consider a special case of the model that will be convenient later.

D. *A Convenient Special Case*

In the special case of the model where the marginal utility of leisure is constant so that $\beta = 1$, the characterization of the equilibrium is much simpler. The relation between real money balance and aggregate demand is still given by equation (5). But, from equation (11) and symmetry, the real wage is constant, so that the price rules are now given by

$$(14) \quad P_i/P = k(M/P)^{(\alpha-1)/(1+\theta(\alpha-1))},$$

where k is an unimportant constant. Equations (5) and (14) characterize the equilibrium. The symmetric equilibrium is identical to that in Figure 1, except for the fact that the aggregate wage rule is horizontal.

This special case is convenient as it allows to concentrate on the interactions between price setters rather than between both price and wage setters. The assumption of constant marginal utility of leisure is unattractive, but equations (5) and (14) admit an alternative interpretation: they can also be derived as the equations characterizing the equilibrium of an economy with many household-firms, each producing a differentiated good with a constant returns technology and increasing marginal disutility of work, and deriving utility from the consumption of all goods and money services.[9] Under that interpretation, $(\alpha-1))$ stands for the elasticity of marginal utility of leisure.

II. Inefficiency and Externalities

A. *Comparing Monopolistic Competition and Perfect Competition*

To characterize the inefficiency associated with monopolistic competition, we first compare the equilibrium to the competitive equilibrium. The competitive equilibrium is derived under the same assumptions about tastes, technology, and the number of firms and households, but assuming that each firm (each household) takes its price (wage) as given when deciding about its output (labor).

The competitive equilibrium is very similar to the monopolistically competitive one. The demand functions for goods and labor are still given by equations (7) and (8). The price and wage rules are identical to equations (10) and (11), except for the absence of $\theta/(\theta-1)$ in the price rules and the absence of $\sigma/(\sigma-1)$ in the wage rules (the K's are the same in both equilibria). The explanation is simple. The term $\theta/(\theta-1)$ is the excess of price over marginal cost, reflecting the degree of monopoly power of firms in the goods market; if firms act competitively, price is instead equal to marginal cost. The same explanation applies to households.

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[9] Laurence Ball and David Romer (1987) derive the equilibrium for such an economy.

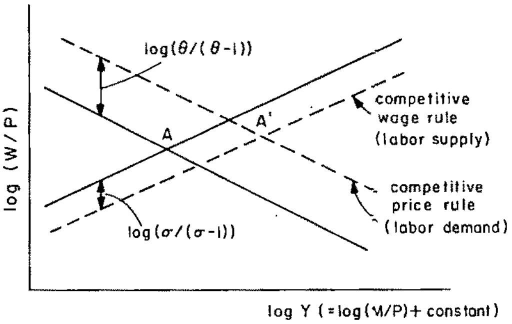

FIGURE 2. MONOPOLISTICALLY COMPETITIVE AND COMPETITIVE EQUILIBRIA

Again, symmetry requires in equilibrium all nominal and all wages to be the same; this gives equations identical to (12) and (13), but without the terms $\theta/(\theta-1)$ and $\sigma/(\sigma-1)$. The price wage ratio consistent with firms' behavior is lower in the competitive case by $\theta/(\theta-1)$ at any level of output; the real wage consistent with household's behavior is lower in the competitive case by $\sigma/(\sigma-1)$ at any level of output. The monopolistically competitive and competitive aggregate wage and price rules are drawn in Figure 2. Point A' gives the competitive equilibrium; point A gives the monopolistically competitive equilibrium.

The equilibrium level of real money balances is lower in the monopolistic equilibrium; the price level is higher. Employment and output are lower. What happens to the real wage is ambiguous and depends on the degrees of monopoly power in the goods and the labor markets. If, for example, there is monopolistic competition in the goods market but perfect competition in the labor market, then the real wage is unambiguously lower under monopolistic competition.

Denoting by R the ratio of output in the monopolistically competitive equilibrium to output in the competitive equilibrium, R is given by

$$R=\left(\frac{\sigma-1}{\sigma}\,\frac{\theta-1}{\theta}\right)^{1/(\alpha\beta-1)}<1,$$

where R is an increasing function of σ and θ. The higher the elasticity of substitution between goods or between types of labor, the closer is the economy to the competitive equilibrium. R is an increasing function of α and β. If α and β are both close to unity, R is small: the existence of monopoly power in either the goods or the labor markets can have a large effect on equilibrium output.[10]

B. *Aggregate Demand Externalities*

Under monopolistic competition, output of monopolistically produced goods is too low. We have shown above that this follows from the existence of monopoly power in price and wage setting. An alternative way of thinking about it is that it follows from an aggregate demand externality.

The argument is as follows. In the monopolistically competitive equilibrium, each price (wage) setter has, given other prices, no incentive to decrease its own price (wage) and increase its output (labor). Suppose however that all price setters decrease their prices simultaneously; this increases real money balances and aggregate demand. The increase in output reduces the initial distortion of underproduction and underemployment and increases social welfare.[11]

We now make the argument more precise. By the definition of a monopolistically competitive equilibrium, no firm has an incentive to decrease its price, and no worker has an incentive to decrease its wage, given other prices and wages. Consider now a proportional decrease in all wages and all prices, $(dP_i/P_i)=(dW_j/w_j)<0$, for all i and j, that leaves all relative prices unchanged but decreases the price level.

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[10] Note that, as there are distortions in two sets of interrelated markets, the economy is operating inside its production frontier. This point is developed by Rotemberg (1982).

[11] An alternative way of stating the argument is as follows: If starting from the monopolistically competitive equilibrium, a firm decreased its price, this would lead to a small decrease in the price level and thus to a small increase in aggregate demand. While the other firms and households would benefit from this increase in aggregate demand, the original firm cannot capture all of these benefits and thus has no incentive to decrease its price. We have chosen to present the argument in the text to facilitate comparison with the argument of Section III.

Consider first the change in the real value of firms.[12] At a given level of output and employment, the real value of each firm is unchanged. The decrease in the price level, however, increases real money balances and aggregate demand. This in turn shifts outward the demand curve faced by each firm and increases profit: an increase in demand at a given relative price increases profit as price exceeds marginal cost. Thus, the real value of each firm increases.

Consider then the effect of a proportional reduction of prices and wages on the utility of each household. Consider household j. We have seen that, once the household has chosen the allocation of his wealth between real money balances and consumption, we can write its utility as

$$U_j = \mu(I_j/P) - N_j^{\beta},$$

where μ is the constant marginal utility of real wealth and I_j is the total wealth of the jth household. Using the budget constraint, we can express utility as

$$U_j = \left[\mu(W_j/P)N_j - N_j^{\beta}\right] + \mu\sum_{i=1}^{m} V_{ij}/P + \mu(M_j/P).$$

Utility is the sum of three terms. The second is profit income in terms of utility; we have seen that each firm's profit goes up after an increase in aggregate demand. Thus, this term increases. The first term is the household's surplus from supplying labor. At a given level of employment N_j, the proportional change in wages and prices leaves this term unchanged. But the increase in aggregate demand and the implied derived increase in employment implies that this term increases: at a given real wage, an outward shift in the demand for labor increases utility as the real wage initially exceeds the marginal utility of leisure. The third term is the real value of the money stock, which increases with the fall in the price level. Thus, utility unambiguously increases.[13]

The aggregate demand externality implies that underproduction is magnified through macroeconomic interactions. Consider the problem faced by an individual firm, with upward-sloping marginal cost and downward-sloping demand, taking as given other prices and aggregate demand. Because the marginal utility of wealth is constant under our assumptions, we can measure welfare by looking at the sum of consumers' and producers' surplus. If the firm acted competitively rather than as a monopolist, the price would be lower and the surplus would be larger. This is the familiar partial-equilibrium effect. But here, in addition, if all firms acted competitively and decreased their prices, aggregate demand would be higher and the demand curve facing each firm would shift to the right and welfare would be further increased. This is the additional general equilibrium effect present here.

Identifying the inefficiency associated with monopolistic competition as an aggregate demand externality does not, however, imply that movements in aggregate demand affect output. Aggregate demand changes associated with changes in the demand elasticities facing firms and workers will have real effects, and these effects are likely to be different under perfect and monopolistic competition. These are not, however, the effects we want to focus on. For that reason, we shall concentrate on changes in nominal money. But then it is clear that, as equations (12) and (13) are homogeneous of degree zero in P, W, and M, nominal money is neutral, affecting all nominal prices and wages proportionately, and leaving output and employment unchanged. Thus something else is needed to obtain real effects of nominal money. We examine the effects of costs of price setting in the next section.

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[12] What happens to the real value of firms is obviously of no direct relevance for welfare. This step is however required to characterize what happens to the utility of households below.

[13] Note that, if we were performing the same experiment in the neighborhood not of the monopolistically competitive but of the competitive equilibrium, the first two terms would be equal to zero. The third, however, would still be present. This is one of the implications of our use of real money as the nonproduced good. If real money enters utility, then the competitive equilibrium is not Pareto optimal, as a small decrease in the price level increases welfare. This inefficiency of the competitive equilibrium disappears if money is replaced by a nonproduced good, while the aggregate demand externality under monopolistic competition remains (see Kiyotaki, 1985a).

III. Menu Costs and Real Effects of Nominal Money

We now introduce small costs of setting prices, small "menu" costs. Akerlof and Yellen (1985a, b) and Mankiw have shown how such small costs can lead to large welfare effects in imperfectly competitive economies.[14] We apply their argument to the specific context of monopolistic competition, derive welfare and output effects as explicit functions of the underlying parameters, and relate these effects to the externality identified earlier.

A. *The Effects of Small Changes in Nominal Money*

We start by considering the effects of a small change in nominal money, dM. The intuitive argument is the following.

At the initial nominal prices and wages, the change in nominal money leads to a change in aggregate demand, thus to a change in the demand facing each firm. If demand is satisfied, the change in output implies in turn a change in the derived demand for labor, thus a change in the demand facing each worker. Unless firms operate under constant returns, each firm wants to change its relative price. Unless workers have constant marginal utility of leisure, each worker wants to change his relative wage. The loss in value to a firm which does not adjust its relative price is, however, of second order; the same is true of the utility of a worker who does not adjust his relative wage. Thus second-order menu costs may prevent firms and workers from adjusting relative prices and wages. The implication is that nominal prices and nominal wages do not adjust to the change in nominal money. The second part of the argument is to show that the change in real money balances has first-order effects on welfare; we show that it is indeed first order, and of the same sign as the change in money. The argument has very much the same structure as the aggregate demand externality argument of the previous section; this coincidence is not accidental and we return to it below.

The first part is a direct application of the envelope theorem. Consider firms first. Let V_i be the value of firm i. V_i is a function of P_i as well as of P, W, and M ($V_i = V_i(P_i, P, W, M)$). Let V_i^* be the maximized value of firm i, after maximization over P_i; $V_i^* = V_i^*(P, W, M)$. The envelope theorem then says that

$$dV_i^*/dM = \partial V_i/\partial M + (\partial V_i/\partial P_i)(dP_i/dM)$$
$$= \partial V_i/\partial M.$$

To a first order, the effect of a change in M on the value of the firm is the same whether or not it adjusts its price optimally in response to the change in M. Exactly the same argument applies to the utility of the household. Thus, second-order menu costs (larger than the second-order loss in utility or in value) will prevent each firm from changing its price given other prices and wages and each worker from changing its wage given other prices and wages. The implication is that all nominal prices and wages remain unchanged, and that the increase in nominal money implies a proportional increase in real money balances.

What remains to be shown is that the increase in real money balances has positive first-order effects on welfare. However, we have already shown in the previous section that the increase in real money balances, associated with the increase in aggregate demand and employment, raises firms' profits and the households' surpluses from supplying labor. Thus, it increases welfare in the neighborhood of the monopolistically competitive equilibrium.

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[14] Akerlof and Yellen assume "near rationality" that is equivalent to rationality subject to second-order costs of taking decisions.

There is clearly a close relation between the *aggregate demand externality and the menu cost argument* of this section. This relation is most simply shown in the special case where the marginal utility of leisure is constant, where there are no menu costs in wage setting and where we can concentrate on the interactions between price setters only. Equation (14), which gives the individual price rules in the absence of menu costs, can be rewritten as

$$(15)\quad P_i/M = k(P/M)^{[1+(\theta-1)(\alpha-1)]/(1+\theta(\alpha-1))}.$$

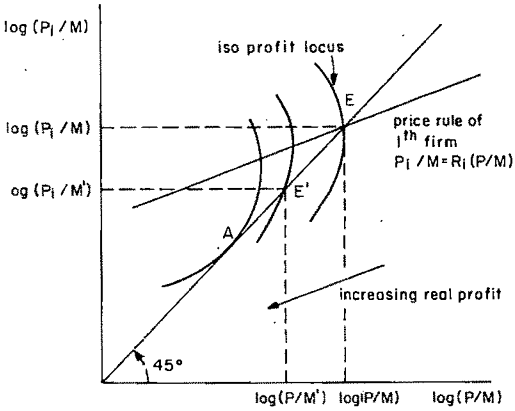

FIGURE 3. AGGREGATE DEMAND EXTERNALITIES AND MENU COSTS

Figure 3 plots the price chosen by firm i as a function of the price level, both as ratios to nominal money. In the presence of monopoly power, the price rule has slope smaller than one. We also draw iso-profit loci, giving combinations of (P_i/M) and (P/M) that yield the same level of real profit for the firm.[15] The symmetric monopolistically competitive equilibrium is given by the intersection of the price rule and the 45° line, point E. Point A gives the highest real profit point on the 45° line.

The aggregate demand externality argument can then be stated as follows. Consider *a small proportional decrease of prices*, keeping nominal money constant. The equilibrium moves from point E to a point like E' along the 45° line. The profit of each firm rises with the increase in aggregate demand. However, in the absence of coordination, no firm has an incentive to reduce prices away from the equilibrium point E.

The menu cost argument considers instead *a small increase in nominal money*. At the initial set of prices, real money balances would increase and the economy would move from point E to a point like point E'. But, absent menu costs, each firm would find in its interest to increase its price until the economy had returned to point E. In the presence of menu costs, however, these menu costs, if large enough, can prevent this movement back to E, so that the economy remains at E' and all firms end up with higher real profits. (A similar argument, although slightly more complicated, holds for the general model. We shall not present it here.)

It is also important to note the *specific role played by money* in this section. The presence of an aggregate demand externality does not depend on the nature of the nonproduced good, or on the nature of the numeraire. The results of this section depend on money being the nonproduced good and the numeraire. That money is the numeraire implies that, given menu costs, unchanged prices and wages mean unchanged nominal prices and wages. That money is the nonproduced goods implies that, as the government can vary the amount of nominal money, it can, if nominal prices and wages do not adjust, change the amount of real money balances, the real quantity of the nonproduced good.

B. *The Effects of Larger Changes in Nominal Money*

For larger changes in money, the private opportunity costs of not adjusting prices in response to the change in money (private costs, for short) are no longer negligible and depend on the parameters of the model. We now investigate this dependence.

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[15] The figure assumes decreasing returns to scale. Note also that, as firms take the price level as given when choosing their own price, iso-profit loci are vertical along the price rule.

The *private costs* faced by a firm depends on the size of the demand shifts as well as on the two parameters α and θ. As we have seen, these costs are of second order in response to a change in aggregate demand, thus roughly proportional to the square of the change in aggregate demand. More precisely, define $L(\Delta;\alpha,\theta)$ to be the private opportunity cost to a firm expressed as a proportion of initial revenues, associated with not adjusting its price in response to a change of $100\Delta\%$ in aggregate demand, when all other firms and households keep their prices and wages unchanged. Then, by simple computation, we get

$$L(\Delta;\alpha,\theta)=\left\{\left[(\alpha-1)^2(\theta-1)\right]\Big/\left[2(1+\theta(\alpha-1))\right]\right\}\Delta^2+0(\Delta^2),$$

where $0(\Delta^2)$ is of third order.

The closer α is to one, that is, the closer to constant returns to scale, the smaller the private cost. In the limit, if α is equal to one, then private costs of not adjusting prices are equal to zero as the optimal response of a monopolist to a multiplicative shift in isoelastic demand under constant marginal cost is to leave the price unchanged. Thus private costs are an increasing function of α. They are also an increasing function of θ; the higher the elasticity of demand with respect to price; the higher the private costs of not adjusting prices.

Exactly the same analysis applies to workers. The two important parameters for them are β and σ. If we define the function L in the same way as above, the private opportunity cost to a worker (measured in terms of consumption and expressed as a proportion of initial consumption), associated with not adjusting the wage in response to a change of $100\Delta\%$ in aggregate demand, when all other firms and households keep their prices and wages unchanged, is given by

$$[(\theta-1)/\theta\alpha]\,L\big((1+\Delta)^{\alpha}-1;\beta,\sigma\big),$$

where $(\theta-1)/\theta\alpha$ is the initial share of wage income in GNP.

If β is close to unity, that is, if the elasticity of the marginal disutility of labor is close to unity, private costs of not adjusting wages are small; in the limit, if marginal disutility of labor is constant, private costs are equal to zero. If σ is very large, if different types of labor are close substitutes, private costs of not adjusting wages are high.

Table 1, Part a, gives the size of menu costs as a proportion of the firm's revenues (GNP produced by the firm) that are just sufficient to prevent a firm from adjusting its price in response to a change in demand; Table 1, Part b, gives the size of menu costs as a proportion of initial consumption (GNP consumed by the worker) that are just sufficient to prevent a worker from adjusting his (her) wage.

Thus, given the unit elasticity of aggregate demand with respect to real money balances and the assumption that all other prices have not changed, Part a of Table 1 gives the private costs associated with not changing prices in the face of 5 and 10 percent changes in demand to the firm. The main conclusion is that very small menu costs, say less than .08 percent of revenues, may be sufficient to prevent adjustment of prices. Results are qualitatively similar for workers. Part b of Table 1 gives the private costs of not changing wages in response to changes of +5 and +10 percent in the demand for goods. It assumes that α is equal to 1.1, so that changes in the derived demand for labor are of 5.5 and 11 percent approximately. We expect β to be higher than α so that Part b of Table 1 looks at values of β between 1.2 and 1.6. For values of β close to unity, required menu costs are again very small; as β increases however, required menu costs become nonnegligible: for $\beta=1.6$ and a 11 percent change in demand, they reach .45 percent of initial consumption, a number which is no longer negligible.

The more relevant comparison, however, at least from the point of view of welfare, is between private costs and *welfare effects*, that is, the change in utility resulting from the changes in output, employment, and real money that are implied by a change in nomi-

TABLE 1—CHANGE IN AGGREGATE DEMAND AND MENU COSTS

(a): Loss in value to a firm from not adjusting prices (as a proportion of initial revenues)[a]				(b): Loss in utility (in terms of consumption) to a worker from not adjusting wages (as a proportion of initial consumption)[a,b]			
		$M_1/M_0 =$				$M_1/M_0 =$	
α	θ	1.05	1.10	β	σ	1.05	1.10
1.0	5	.000	.000	1.2	5	.025	.100
1.1	5	.003	.013	1.4	5	.066	.265
1.1	2	.001	.004	1.4	2	.027	.111
	20	.008	.031		20	.105	.418
1.3	5	.018	.071	1.6	5	.112	.451

Note: M_0 is the initial level of nominal money, M_1 the level after the change.

[a] Shown in percent.

[b] $\theta = 5$; $\alpha = 1.1$.

TABLE 2—MENU COSTS AND WELFARE EFFECTS

		$M_1/M_0 = 1.05$			$M_1/M_0 = 1.10$		
α	β	Menu Costs[a]	Welfare Effects[a]	Ratio	Menu Costs[a]	Welfare Effects[a]	Ratio
1.1	1.2	.03	1.79	60	.11	3.54	32
($\theta = \sigma = 5$)	1.4	.07	1.83	26	.28	3.60	13
	1.6	.11	1.91	17	.46	3.72	8
1.2	1.2	.04	1.82	45	.15	3.57	24
	1.4	.08	1.87	24	.33	3.67	11
	1.6	.13	1.98	15	.53	3.85	7
1.1	1.2	.03	.94	31	.11	1.86	17
($\theta = \sigma = 10$)	1.4	.06	1.02	17	.23	1.93	8
	1.6	.09	1.11	12	.36	2.05	6
1.2	1.2	.04	.99	25	.16	1.87	12
	1.4	.07	1.07	16	.29	2.01	7
	1.6	.11	1.27	12	.44	2.24	5

[a] Shown in percent.

nal money at given prices and wages. Welfare effects depend on the size of the change in nominal money as well as on the parameters α, β, θ, and σ; the dependence is a complex one and we shall not analyze it here in detail. Table 2 gives numerical examples. It gives the required menu costs and welfare effects associated with two different changes in nominal money, 5 and 10 percent, and different values of the structural parameters.

For each of the two changes in money, the first column gives the minimum value of menu costs, expressed as a proportion of GNP, that prevents adjustment of nominal prices and wages; this value is the sum of menu costs required to prevent firms from adjusting their prices and workers from adjusting their wages, given other wages and prices. The second column gives the welfare effects of an increase in nominal money at unchanged prices and wages, expressed in terms of consumption, again as a proportion of GNP. The third gives the ratio of welfare effects to menu costs.

Welfare effects turn out not to be much affected by the specific values of the parameters, at least for the range of values we consider in the table. Thus, the ratio of welfare effects to menu cost has the same qualitative behavior as that of the ratio of output movements to menu costs. It is largest for values of α, β, θ, and σ close to unity, and decreases as these parameters increase. In the table, it varies from 60 for low values of α, β, θ and σ to 5 for high values of these parameters.

C. *Strategic Complementarities and Multiple Equilibria*

We have shown that, if menu costs were sufficiently large, there was an equilibrium where, in response to an increase in nominal money, nominal prices and wages remained unchanged and nominal money had real effects. What we have not shown, however, is that this was the only equilibrium. It turns out that, if the condition that Cooper and John have called "strategic complementarity" is satisfied, there exists a range of menu costs for which the equilibrium is not unique. This has been shown by Julio Rotemberg and Garth Saloner (1986a, b) and, in the context of this model, by Laurence Ball and David Romer (1987). We give a brief sketch of their argument but refer the reader to those papers for details.

To discuss the existence and the role of strategic complementarity in this context, it is again easiest to study the special case where the marginal utility of leisure is constant and where there are no menu costs in nominal wage setting, so that real wage is constant and we can concentrate on the interactions between price setters. Following Cooper and John's definition, strategic complementarity corresponds then to the case where an increase in the price level leads a firm, absent menu costs, to increase its own nominal price. Strategic substitutability corresponds to the case where an increase in the price level leads the firm instead to decrease its nominal price.

In the special case we are considering, the price that each firm would choose, absent menu costs, is given by equation (14). An increase in P has two effects on P_i. At a given level of aggregate demand, the firm wants to keep the same relative price, and increase P_i in proportion to P. But an increase in the price level also decreases real money balances and aggregate demand, leading the firm to want a decrease in its relative price. The net effect is, in our context, unambiguous: the relative price effect dominates the aggregate demand effect, so that an increase in P leads to an increase in P_i, although less than one for one. Thus, the model exhibits strategic complementarity. This is not, however, a robust feature of the model: if the elasticity of aggregate demand with respect to real money balances exceeded one, the aggregate demand effect could dominate the relative price effect; an increase in P could lead the firm to decrease rather than increase P_i and the model would exhibit instead strategic substitutability. It is clear that the results derived above on the inefficiency of the monopolistically competitive equilibrium, the existence of an aggregate demand externality and the role of menu costs do not depend on whether the model exhibits strategic complementarity or substitutability. But, if the model exhibits strategic complementarity however, the equilibrium with menu costs may not be unique.

Consider what a particular firm should do after an increase in nominal money. If other firms do adjust their prices, and if prices are strategic complements, the firm's optimal price, P_i, increases because of both the increase in M and the increase in P. If, instead, other firms do not adjust their prices, so that the price level does not change, the optimal price P_i increases, but by less. Put another way, the larger the proportion of firms which do not adjust their prices, the lower the opportunity cost to a firm of not adjusting its price. Thus, for some values of the menu costs, there will be two equilibria, one in which all firms adjust, making it costly for a given firm not to adjust, and the other in which no firm adjusts, making it less costly for a given firm not to adjust.

Strategic complementarity can therefore lead to multiple equilibria in the context of monopolistic competition with menu costs.[16] These equilibria are associated with different levels of welfare, and to the extent that the economy ends at a low welfare equilibrium, this can be seen as a "coordination failure," the term used by Cooper and John.

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[16] Strategic complementarity turns out to play an important role in dynamic models with menu costs. See the surveys by Blanchard (1987) and Rotemberg (1987).

D. *Demand Determination of Output*

We have until now assumed that increases in real money balances at constant prices and wages led to increases in output and employment. When analyzing the effects of small changes in money, this assumption was clearly warranted; in the initial equilibrium, as price exceeds marginal cost, firms will always be willing to satisfy a small increase in demand at the existing price. The same is true of workers: as the real wage initially exceeds the marginal disutility of labor, workers will willingly accommodate a small increase in demand for their type of labor. When we consider larger changes in money, this may no longer be the case. Even if firms do not adjust their price, they have the option of either accommodating or rationing demand; they will resort to the second option if marginal cost exceeds price. The same analysis applies to workers. From standard monopoly theory, we know that firms and workers will accommodate relative increases in demand, respectively, of

$$(\theta/(\theta-1))^{1/(\alpha-1)}$$

and $$(\sigma/(\sigma-1))^{1/(\beta-1)}.$$

This raises the question of whether, assuming menu costs to be large enough, an increase in demand can increase output all the way to its competitive level. The answer is provided in Figure 4. This figure replicates Figure 2 and draws the aggregate price and wage rules under competitive and monopolistically competitive conditions. Point A is the monopolistic competitive equilibrium and A' the competitive one. Along the monopolistically competitive price rule, price exceeds marginal cost; thus firms will satisfy demand, at a given price wage ratio, until marginal cost equals price, that is, until they reach the competitive locus. In our case, firms will supply up to point B. The shaded area F is the set of output-real wage at which firms will ration rather than supply.

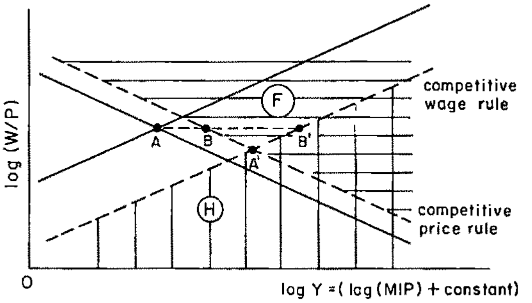

FIGURE 4. DEMAND DETERMINATION OF OUTPUT

By a similar argument, workers will supply up to point B'. The shaded area H is the set of real wage combinations where workers do not satisfy labor demand. The figure makes it clear that an increase in nominal money will increase output and employment. It also makes clear that, no matter how large menu costs are, it is impossible, unless the competitive and monopolistically competitive real wages are equal, to attain the competitive equilibrium through an increase in nominal money.

What happens, therefore, as demand increases depends on both menu costs and supply constraints. If menu costs are large, supply constraints will come into effect first. If menu costs are small, a more likely case, prices and wages adjust before supply constraints come into effect.

IV. Fixed Costs and Aggregate Demand Movements

Until now, we have taken the number of firms as given and fixed costs have played no role in our analysis. Both the aggregate demand externality and the menu costs arguments hold, irrespective of the presence of fixed costs.

In this section, we want to study further the effects of aggregate demand on activity, taking as given that, because of menu costs, such movements in aggregate demand affect output. It is then essential to introduce fixed costs, as they have important implications for productivity, profitability, and the potential for entry by new firms in response to movements in output induced by aggregate demand.

A. *Fixed Costs and the General Equilibrium*

To introduce fixed costs, we change the specification of the production function to

$$(1') \quad Y_i + F = \left(\sum_{j=1}^{n} N_{ij}^{(\sigma-1)/\sigma} \right)^{\sigma/(\sigma-1)},$$

where F can be interpreted as being in terms of the firm's own output, or as overhead labor requiring all types of labor in the same proportions as regular production.[17] For convenience, we also assume that, apart from the fixed cost, firms operate under constant returns to scale, so that $\alpha = 1$.

Given this specification of fixed costs, equations characterizing equilibrium are very similar to equations (5) to (13). The relation between real money balances and aggregate demand is unchanged. The demand functions for goods are still given by equation (7), while in the demand functions for labor, equation (8), Y is replaced by $(Y+mF)$. The price rules are still given by equation (10), which becomes under the assumption that α is equal to unity:

$$(10') \quad (P_i/P) = (\theta/(\theta-1))K_p(W/P) \qquad i = 1,\dots,m.$$

The wage rules are given by equation (11) where Y is replaced by $(Y+mF)$.

Equations (12) and (13) which give the aggregate price and wage rules become

$$(12') \quad (P/W) = (\theta/(\theta-1))K_p$$

$$(13') \quad (W/P) = (\sigma/(\sigma-1))K_w (Y+mF)^{\beta-1}.$$

Together they determine as before equilibrium aggregate output and real wages. From the assumption of constant returns in this section, the markup, or its inverse the real wage, is independent of the level of demand and is determined by (12′). Given the real wage, (13′) determines the level of output.

We can now go further and determine the equilibrium number of firms. For a given number of firms, m, the real profit of each firm after profit maximization is given by

$$(16) \quad V_i/P = K_v(1/m)(W/P)^{1-\theta}Y - K_p(W/P)F,$$

where K_v and K_p depend on the structural parameters but not on the number of firms. An increase in m has two effects on the profit of each firm. Given Y and (W/P), it decreases the sales of each firm and decreases profit: this effect is captured by the term $(1/m)$ in (16). But it also affects the equilibrium value of Y; while the constants K_p and K_w in (12′) and (13′) do not depend on m, the level of employment associated with a given level of output increases with aggregate fixed costs and therefore with m. This leads to a lower equilibrium value of Y; given the assumption of constant returns, and given that K_p is independent of m, the value of (W/P) is unaffected by m; lower aggregate output in turn decreases profit.

Assuming that entry takes place until profit is equal to zero, equations (12′) to (16) and the zero-profit condition determine the equilibrium number of firms. We shall assume in what follows that the number of firms is initially given by zero-profit conditions but is going to be fixed in the short run.

B. *Movements in Aggregate Demand, Profit, and Entry*

Aggregate demand depends on real money balances, the ratio of nominal money to the price level. Under constant returns with a fixed markup of prices on wages, aggregate demand is therefore a function of the ratio of nominal money to the nominal wage, money in wage units. We now consider the effects of movements in (M/W). This may be due to a movement in nominal money, with no adjustment in nominal wages because of menu costs as in the previous section. Or it may be due to a change in the nominal wage given nominal money; the reason need not concern us in this section. Movements in (M/W) lead to one for one movements in aggregate demand and employment.

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[17] An alternative is to assume that fixed costs for a given firm are in the form of other goods. A convenient assumption is to assume that the fixed cost is a CES function of all goods, with elasticity of substitution θ. This assumption insures that the demand functions for goods still have elasticity θ.

The first obvious implication of fixed costs is that productivity, given by $(Y/(Y+mF))$, varies procyclically. We focus on a related implication, the procyclical behavior of profit. From equation (16), profit is, given that nominal wages and prices are fixed, an increasing function of output. If profit was initially equal to zero, it becomes negative with a decrease in (M/W), positive with an increase in (M/W). Both procyclical productivity and profitability seem to be in accordance with the facts.

Procyclical profitability has interesting implications for entry. Keeping the number of firms constant, we examine the return to entry as a function of aggregate demand.[18] Consider the effects of a decrease in nominal money in wage units and the associated reduction in output. Existing firms make negative profits and thus, if a potential entrant has to pay the prevailing wage, it will not want to enter. We can go further and derive the shadow wage, say W^*, which would make a potential entrant indifferent to entry, as a function of (M/W). Going back to equation (5), (12′), and (16), given M and the aggregate nominal wage W, if a firm can pay its workers W^*, it will make profits of

$$\left[K(K_v/m)\left((\theta-1)/\theta K_p\right)^{\theta-2}\right]$$
$$\times(W^*/W)^{1-\theta}(M/W)$$
$$-[(\theta-1)/\theta](W^*/W)F].$$

For a potential entrant to be indifferent to entry, this expression has to be equal to zero. Assuming zero profit for the potential entrant, we can derive the elasticity of the relative wage (W^*/W) to the money stock in wage units (M/W). Denoting it by η, we have

$$\eta=(1/\theta).$$

Equivalently the elasticity of the shadow nominal wage to the aggregate nominal wage is given by $1-(1/\theta)$.[19] Thus, the higher are aggregate nominal wages, the more difficult is entry, the lower the relative wage a new entrant can offer to its workers. This result depends on both the presence of monopolistic competition and fixed costs. Fixed costs imply that when higher nominal wages decrease aggregate demand, the profit of all firms can be negative; the fact that products are imperfect substitutes implies that new entrants face downward-sloping demands and cannot capture all of demand by simply undercutting the prices of existing firms.

This result has two implications. In the previous section, we showed that, if the number of firms was given, menu costs could lead firms not to change prices in response to changes in nominal money. We see that, with respect to decreases in money, allowing for entry will not change the result. More generally (and stepping outside our model) this result implies that if nominal wages are too high, perhaps because of union push, and lead to unemployment, new firms will not want to enter, even if they can pay wages to the unemployed that are far below those paid to the employed.[20] Absent fixed costs, entry would occur and unemployment subside.[21]

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[18] An alternative would be to determine the equilibrium number of firms as a function of the level of aggregate demand. This is the route pursued by Robert Solow (1984).

[19] As θ is greater than unity, the elasticity of the shadow wage with respect to the aggregate wage is positive. If we had, however, formalized fixed costs in terms of a basket of goods, the sign of the elasticity would depend on whether θ is greater or smaller than 2. The difference comes from the fact that, in our case, a decrease in the shadow wage decreases the fixed cost whereas, under this alternative assumption, it does not.

[20] We are indebted to Larry Summers for suggesting this argument.

V. Conclusion

The results of this paper are tantalizingly close to those of traditional Keynesian models: under monopolistic competition, output is too low, because of an aggregate demand externality. This externality, together with small menu costs, implies that movements in demand can affect output and welfare. In particular, increases in nominal money can increase both output and welfare. In the presence of fixed costs, output, productivity, and profitability move in the same direction.

We believe these results to be important to the understanding of macroeconomic fluctuations; we want to point out the obvious limitations of the analysis as it stands.

The scope for small menu costs to lead to large output effects in our model depends critically on the elasticity of labor supply with respect to the real wage being large enough (on $(\beta-1)$ being small). Evidence on individual labor supply suggests however a small elasticity. Thus the "menu cost" approach runs into the same problem as the imperfect information approach to output fluctuations; neither can easily generate large fluctuations in output in response to demand if the real wage elasticity of labor supply is low. As in the imperfect information case, the theory may be rescued by the distinction between temporary and permanent changes in demand. Another possibility is that unions have a flatter labor supply than individuals, or do not represent the whole labor force. More likely, the assumption that labor markets operate as spot markets (competitive or monopolistically competitive) will have to be abandoned.[22]

The analysis of this paper is purely static. There are two main issues involved in extending the model to look at the dynamic effects of aggregate demand on output in the presence of menu costs.

The first is that we have assumed all prices to be initially equal and set optimally. In a dynamic economy and in the presence of menu costs, such a degenerate price distribution is unlikely. But, if prices are initially not all equal or optimal, it is no longer obvious that even a small change in nominal money will leave all prices unaffected. It is no longer obvious that money, or aggregate demand in general, will have large effects on output.

The second is that, even if nominal money has large effects on output, it must be the case that money is sometimes unanticipatedly high, sometimes unanticipatedly low. When money is high, output increases and so does welfare to a first order. When money is low, output decreases and so does welfare, again to a first order. These welfare effects would appear to cancel out to a first order. It is therefore no longer obvious that, even if menu costs lead to large output fluctuations, the welfare loss of those fluctuations exceeds the menu costs which generate them.

Fortunately, all these issues are the subject of active research. Recent developments are reviewed in Blanchard and Rotemberg.

APPENDIX

This Appendix derives the market equilibrium conditions (5) to (11) given in the text. It proceeds in three steps. The first derives the demand functions of each type of labor and each type of product by solving part of the maximization problems of firms and households. These functions hold whether or not prices and wages are set by workers and firms at their profit or utility maximizing level. The second derives price rules from firms' profit maximization and wage rules from workers' utility maximization. The third characterizes market equilibrium.

A. *Demands for Product and Labor Types*

(a) *Demand for product of type i.* In order to maximize utility, each household chooses the optimal composition of consumption and money holdings for a given level of total wealth I_j and product prices:

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[21] This is our interpretation of Weitzman's argument that increasing returns are a necessary condition for unemployment to persist.

[22] This is indeed the direction taken by Akerlof and Yellen (1985b) who formalize the goods market as monopolistically competitive and the labor market using the "efficiency wage" hypothesis.

$$\max_{C_{ij},\, M'_j} \Lambda_j = \left(\sum_{i=1}^{m} C_{ij}^{(\theta-1)/\theta}\right)^{\theta\gamma/(\theta-1)}$$

$$\times m^{\gamma/(1-\theta)}\left(M'_j/P\right)^{1-\gamma}$$

$$\text{subject to} \quad \sum_{i=1}^{m} P_i C_{ij} + M'_j = I_j.$$

Solving this maximization problem gives

$$\text{(A1)} \quad C_{ij} = (P_i/P)^{-\theta}(\gamma I_j/mP)$$

$$\text{(A2)} \quad M'_j = (1-\gamma)I_j$$

$$\text{(A3)} \quad \Lambda_j = \mu I_j/P$$

where

$$\text{(A4)} \quad P = \left((1/m)\sum_{i=1}^{m} P_i^{1-\theta}\right)^{1/(1-\theta)} \quad \text{and}$$

$$\mu = \gamma^{\gamma}(1-\gamma)^{1-\gamma},$$

μ can be interpreted as the marginal utility of real wealth.

The demand for product of type i is therefore given by

$$\text{(A5)} \quad Y_i = \sum_{j=1}^{n} C_{ij} = (P_i/P)^{-\theta}(Y/m),$$

where

$$\text{(A6)} \quad Y = \left(\sum_{j}^{n}\sum_{i}^{m} P_i C_{ij}\right)/P = (\gamma/P)\sum_{j=1}^{n} I_j.$$

Y denotes real aggregate consumption expenditures of households and will be referred to as "aggregate demand." Equation (A5) is equation (7) in the text. Note also that equations (A1), (A2), (A5), and (A6) imply the following relation between aggregate demand and aggregate desired real money balances:

$$\text{(A7)} \quad Y = (\gamma/(1-\gamma))M'/P$$

$$\text{where} \quad M' = \sum_{j=1}^{n} M'_j.$$

(b) *Demand for labor of type j.* In order to maximize profit, each firm minimizes its production cost for a given level of output and wages:

$$\min_{N_{ij}} \sum_{j=1}^{n} W_j N_{ij}$$

$$\text{subject to} \quad \left(\sum_{j=1}^{n} N_{ij}^{(\sigma-1)/\sigma}\right)^{(\sigma/(\sigma-1))1/\alpha} = Y_i.$$

Solving this minimization problem gives

$$N_{ij} = \left(n^{\sigma/(1-\sigma)}\right)(W_j/W)^{-\sigma}Y_i^{\alpha}$$

and

$$\text{(A8)} \quad \sum_{j=1}^{n} W_j N_{ij} = n^{\sigma/(1-\sigma)} W Y_i^{\alpha}$$

where

$$\text{(A9)} \quad W = \left((1/n)\sum_{j=1}^{n} W_j^{1-\sigma}\right)^{1/(1-\sigma)}.$$

The demand for labor of type j is therefore given by

$$\text{(A10)} \quad N_j = \sum_{i=1}^{m} N_{ij} = (W_j/W)^{-\sigma}N/n,$$

where

$$\text{(A11)} \quad N \equiv \left(\sum_{i}^{m}\sum_{j}^{n} W_j N_{ij}\right)/W$$

$$= n^{1/(1-\sigma)}\sum_{i=1}^{m} Y_i^{\alpha}.$$

N can be interpreted as the aggregate labor demand index.

B. *Price and Wage Rules*

(a) Taking as given wages and the price level, each firm chooses its price and output so as to maximize profit:

$$\text{(A12)}\qquad V_i = P_iY_i - \sum_{j=1}^{n} W_jN_{ij},$$

subject to the cost function (A8) and the demand function for its product (A5). Solving the above maximization problem gives

$$\text{(A13)}\quad P_i/P = \left[\left((\theta/(\theta-1))n^{1/(1-\sigma)}\alpha m^{1-\alpha}\right) \times (W/P)Y^{\alpha-1}\right]^{1/(1+\theta(\alpha-1))}.$$

Equation (A13) implies that the price is equal to $\theta/(\theta-1)$ times the marginal cost. Equation (A13) is equation (10) in the text.

(b) Taking as given prices and other wages, each household chooses its wage and labor supply so as to maximize utility. Using (A3),

$$\text{(A14)}\qquad U_j = \mu I_j/P - N_j^{\beta}$$

subject to the demand for its type of labor (A10) and the budget constraint:

$$\text{(A15)}\qquad I_j = W_jN_j + \sum_{i=1}^{m} V_{ij} + M_j.$$

Solving this maximization problem gives

$$\text{(A16)}\quad W_j/W = \left[\left((\sigma/(\sigma-1))(\beta/\mu)n^{1-\beta}\right) \times (P/W)N^{\beta-1}\right]^{1/(1+\sigma(\beta-1))}.$$

Equation (A16) implies that the real wage, in terms of utility, is equal to $\sigma/(\sigma-1)$ times the marginal disutility of labor.

C. *Market Equilibrium*

In equilibrium, desired real money balances must be equal to actual balances. Thus $M = M'$. Replacing in (A7) gives

$$\text{(A17)}\qquad Y = (\gamma/(1-\gamma))M/P.$$

This gives equation (5) in the text.

Replacing the Y_i's in (A11) by their value from (A5) gives

$$\text{(A18)}\quad N = [n^{1/(1-\sigma)}m^{-\alpha}] \times \left(\sum_{i=1}^{m}(P_i/P)^{-\alpha\theta}\right)Y^{\alpha}.$$

If all firms choose the same (not necessarily optimal) price, this reduces to

$$\text{(A19)}\qquad N = [n^{1/(1-\sigma)}m^{1-\alpha}]Y^{\alpha}.$$

Substituting N from equation (A19) into (A10) gives the demand function for labor j, equation (8) in the text. Note that the demand functions for goods and labor, and the relation between aggregate demand and real money balances, have been derived without use of the price and wage rules and therefore hold whether or not wages and prices are set optimally.

Substituting N from equation (A19) into equation (A16) gives the wage rule, equation (11) in the text. This completes the derivation.

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Inventories and the Volatility of Production

By JAMES A. KAHN*

A stylized fact associated with inventory behavior is that the variance of production exceeds the variance of sales. This paper presents a model of production decisions with demand uncertainty that incorporates nonnegativity constraints on inventories. Even with no productivity shocks, optimal behavior by the firm is consistent with this stylized fact, either if demand exhibits positive serial correlation, or if the firm can backlog excess demand.

The importance of changes in inventory investment in cyclical fluctuations has been widely documented. In eight postwar recessions, declines in inventory investment have on average accounted for more than 30 percent of the peak-to-trough declines (relative to trend) of real GNP, even though inventory investment itself represents a much smaller component of GNP.[1] Yet the common assumption that firms try to smooth production in the face of fluctuating demand—because of either diminishing returns or costs of adjusting production—suggests that inventories could be a stabilizing force. According to this view, a temporary decline in demand, for example, would result in some net accumulation of inventories as the firm only partially adjusts its rate of production.

It is safe to say that few economists still believe that finished goods inventories are held only to smooth production in the face of fluctuating demand. The simple fact that the variance of production has been found to exceed the variance of sales—even after controlling for obvious seasonal factors influencing production (for example, Olivier Blanchard, 1983)—strongly suggests that production smoothing is of secondary importance. To account for this stylized fact (which will be referred to below as production "counter-smoothing"), two general approaches have been tried: allowing for a stockout-avoidance motive arising from a nonnegativity constraint on inventories, and allowing for productivity or cost shocks. In the spirit of the first approach is the inclusion in a linear-quadratic model of a (quadratic) cost of being away from some positive target level of inventory. If the target is proportional to expected next-period sales, for example, then the variance of production can exceed the variance of sales. The difficulties with this approach, apart from its obviously *ad hoc* nature, are, first, that it does not prevent inventories from being negative, and, second, that the more general variance inequality that it implies has also been rejected empirically (see Kenneth West, 1986). Also, Andrew Abel's (1985) model with a more rigorous treatment of the nonnegativity constraint still implies that production should be smoother than sales. The approach that allows for cost shocks, as exemplified by Martin Eichenbaum (1984) and Alan Blinder (1986), while able to account for production volatility, in practice still ignores the nonnegativity constraint. It also suffers from the lack of better direct evidence of cost shocks.

This paper develops a model of production and inventory behavior that focuses exclusively on the stockout-avoidance motive. The model potentially explains many of the

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[1] See, for example, Robert Barro (1987, ch. 9). Alan Blinder and Stanley Fischer's (1981) detailed accounting of the 1973–75 recession also suggests that most of the action in inventories occurs in the finished goods component.

stylized facts about inventory behavior that have been documented in recent years, including the "excess" volatility of production (for example, Blinder, 1986, or West, 1986), and the positive correlation between inventory levels and sales (Julio Rotemberg and Garth Saloner, 1985). Inventories arise as a result of two plausible technological constraints: 1) Firms cannot short sell their products without cost; and 2) production decisions are made in advance, before the state of demand is fully known.[2] The model has linear costs, so that there is no conventional production-smoothing motive. The solutions to the firm's optimization problem explicitly take into account the nonnegativity constraint on inventories (i.e., the possibility of stockouts) and a range of assumptions about the firm's ability to backlog its excess demand.

The conditions under which the variance of production exceeds the variance of sales turn out to be quite broad. Demand may be independently and identically distributed or positively serially correlated. Although the linear cost structure presumably overstates the degree of production volatility, the strength of the findings below suggests that production can be more variable than sales even if there is significant convexity of costs, and even if the only source of uncertainty is on the demand side. Hence the model is capable of rationalizing the violations of the production-smoothing models found by Blinder (1986) and West (1986). The model also suggests some ways in which the linear-quadratic framework employed in much empirical research (for example, 1986, and Blanchard, 1983) might be modified to make it a better approximation to more rigorous models.

The plan of the paper is as follows. Section I illustrates the main points of the paper with an extended example. Section II presents the general version of the model, and solves for the firm's optimal price and production decisions. Section III contains the central results of the paper, the conditions under which production will be more variable than sales. The final section summarizes the results and suggests areas for further research.

I. Stockouts and Backlogs: An Extended Example

This section presents two simple cases that highlight the main points of the paper, which are that positive serial correlation in demand and/or a firm's ability to backlog excess demand can lead to production countersmoothing. In this section I consider only the production decision of the firm, and assume that the output price is constant. Section II will consider the pricing decision as well.

A. *Serially Correlated Demand*

Suppose that the firm's demand, denoted x, is and $AR(1)$ process:

$$x_t = d + \rho x_{t-1} + u_t, \tag{1}$$

where $-1 < \rho < 1$. The forecast error u_t is an independently and identically normally distributed random variable with mean zero and variance σ^2, and is uncorrelated with anything known at time $t-1$. The maximum amount the firm can sell in period t is its inventory as of the end of the previous period, denoted by n_{t-1}, plus whatever it produces during period t, y_t. Assuming that the price p is sufficiently high, we have

$$z_t = \min[n_{t-1} + y_t, x_t], \tag{2}$$

where z_t denotes actual sales in period t. The cost of producing y_t is cy_t, where c is a known constant. Profits in period t are simply revenues minus costs $pz_t - cy_t$. The problem for the firm is to choose production each period to maximize the present discounted value of profits

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[2] In my dissertation (1986, ch. 3) the second assumption is weakened to allow for revisions to planned production (with increasing marginal cost) when demand information becomes available. The qualitative features of the model remain the same, except that actual production ends up deviating from planned production by some amount that is correlated with the demand forecast error.

$$E_{t-1}\left[\sum_{s=t}^{\infty}\beta^{s-t}(pz_s - cy_s)\right],$$

subject to the constraints given by the stock-flow identity $n_t = n_{t-1} + y_t - z_t$, demand (1), and the nonnegativity constraint (2).

Dynamic programming techniques yield the following first-order condition:

$$(3)\qquad 0 = (p-c)(1-\Phi(A_t/\sigma)) - c(1-\beta)\Phi(A_t/\sigma),$$

where $A_t = (n_{t-1} + y_t) - E_{t-1}x_t$ (the difference between the amount the firm makes available for sale and its expected demand), $\Phi(\ )$ is the standard normal cumulative distribution function, and β is the discount factor. Interpretation of this condition is reasonably straightforward. First, note that the firm will stock out if and only if $u_t > A_t$; thus $1-\Phi$ is the stockout probability. In the event of a stockout, the gain from having produced an additional unit is $p-c$; in the event that there is stock left over at the end of the period, the firm is worse off as a result of having produced the additional unit by an amount $c(1-\beta)$, since it could have delayed the expenditure by one period. At the optimum, the expected marginal gain must equal zero.

Denoting the stock available for sale $n_{t-1} + y_t$ by a_t, and letting $k = \sigma\Phi^{-1}((p-c)/(p-\beta c))$, we get from (3) the decision rule

$$(4)\qquad A_t = k$$

or

$$(5)\qquad a_t = d + \rho x_{t-1} + k.$$

That is, the optimal a_t equals expected demand plus a factor that depends on the markup, the discount factor, and the variance of the forecast error. These are the parameters that determine the cost of stocking out relative to carrying over inventories. We can use this rule together with (2) to solve for the behavior of production, sales, and inventories in terms of the characteristics of the demand process:

$$(6)\qquad y_t = d + \rho x_{t-1} + \min[u_{t-1}, k].$$

$$(7)\qquad z_t = d + \rho x_{t-1} + \min[u_t, k].$$

$$(8)\qquad n_t = k - \min[k, u_t].$$

Thus production and sales follow similar processes. The important difference, however, is that u_t is correlated with x_t but is uncorrelated with previous realizations of x. Letting $v_t = \min[u_t, k]$, we have

$$(9)\qquad y_t = d + \rho x_{t-1} + v_{t-1},$$

which implies that

$$(10)\qquad \operatorname{var}(y) = \rho^2\sigma^2/(1-\rho^2) + \operatorname{var}(v) + 2\rho\operatorname{cov}(u,v).$$

The same calculations for the sales process yield

$$(11)\qquad \operatorname{var}(z) = \sigma^2\rho^2/(1-\rho^2) + \operatorname{var}(v).$$

Since the covariance of v and u is positive, it is clear that production is more variable than sales in this example if and only if $\rho > 0$.

The intuition for this result is that if demand exhibits positive serial correlation, production will respond to an innovation in sales by more than one-for-one. A positive demand shock has two effects: it reduces inventories one-for-one (at least up to the point of stocking out), *and* it increases expected demand. The first effect alone would make production exactly as variable as sales; the changes in optimal a_t that result from changes in expected demand cause production to be more variable than sales, the more so the more positively serially correlated is demand.

B. *Backlogs*

In this section I set ρ from equation (1) equal to zero so that demand is independently and identically distributed but I allow the firm to backlog its excess demand. The total demand facing the firm in each period now includes any excess demand that may be left over from previous periods. The conditional expectation of "new" demand (that is, demand exclusive of backlogs) is now assumed to be constant. We have

$$x_t = d + (x_{t-1} - z_{t-1}) + u_t. \tag{12}$$

The term in parentheses equals zero in the event that the firm has inventory leftover from the previous period; otherwise it is positive. The firm knows $x_{t-1} - z_{t-1}$ when it chooses y_t.

In this case the first-order condition for optimal production turns out to be

$$0 = (p-c)[1-\Phi(A_t/\sigma)] - c\Phi(A_t/\sigma). \tag{13}$$

The difference between this condition and (3) in Section IA is that now the gain to the firm from having produced an additional unit, conditional on stocking out, is the difference between selling this period and selling next period, $(p-c)(1-\beta)$. In the previous case, excess demand was assumed to be lost forever, whereas with backlogging, the sales are only delayed by one period. Note that the loss from having produced an additional unit in the event of a shortfall in demand is also proportional to $1-\beta$, so that the discount factor β actually does not affect the production decision in this case.

As before, the firm's decision rule takes the following form:

$$A_t = \kappa \tag{14}$$

or

$$a_t = d + (x_{t-1} - z_{t-1}) + \kappa, \tag{15}$$

where $\kappa = \sigma\Phi^{-1}((p-c)/p)$. *Ceteris paribus*, κ is less than k as defined in the previous example, again because the cost of stocking out is smaller in this case.

Given the equation for sales (2), and defining $\nu_t \equiv \min[u_t, \kappa]$ as before, we have

$$z_t = d + (x_{t-1} - z_{t-1}) + \nu_t. \tag{16}$$

End-of-period inventories are then

$$n_t = \kappa - \nu_t, \tag{17}$$

and excess demand in period t can be expressed as

$$x_t - z_t = u_t - \nu_t, \tag{18}$$

which is simply the difference between the forecast error and the truncated forecast error for period t. For u_t small, excess demand is zero, but for $u_t > \kappa$, it is positive.

We can use (14)–(18) to solve for production and sales:

$$y_t = d + u_{t-1} \tag{19}$$

$$z_t = d + (u_{t-1} - \nu_{t-1}) + \nu_t. \tag{20}$$

But the quantity $d + u_{t-1}$ is exactly equal to lagged new demand, and therefore production is exactly as variable as new demand. Sales will clearly be less variable than new demand, though, since when demand is unexpectedly high, sales are shifted to subsequent periods, and indeed one may show rigorously that the variance of (19) is greater than the variance of (20).

These special cases highlight the two sources of production counter-smoothing considered in this paper: serial correlation in demand, and the backlogging of excess demand. The two are clearly related, since backlogs impart some serial correlation to the *total* demand faced by the firm. A large positive shock to demand in one period, for example, is carried over into the next period's total demand by means of the backlog. The two sources are conceptually different, though, so it is useful to consider them separately. Backlogs are negligible or nonexistent in many industries that have significant serial correlation in demand.

The next two sections consider a more general model in which the firm optimally sets price as well as production each period, and may only be able to backlog some portion of its excess demand.

II. The Model

This section considers the more general problem of a monopolist's choice of price and production when it is uncertain about its demand. Each period the firm must decide how much it wishes to have available for sale in the coming period, and at what price, prior to knowing demand for that period.[3] As before, if actual sales turn out to be less than the available stock, the firm may carry the remainder (at some cost) into the next period as inventory. If, on the other hand, the firm "stocks out," the result is lost and/or delayed sales.[4] Except for the inclusion of backlogs, the model of the firm is similar to that of Karlin and Carr (1962).

Given its current information, the firm chooses a price and a quantity of production for the succeeding period to maximize expected profits over an infinite horizon. After observing the realization of demand for that period and any other relevant news, the firm again chooses price and quantity for the next period, and so on. The stock of goods that the firm has available for sale each period is equal to its leftover inventory plus that period's production. Actual sales equal either that period's total demand or the amount available for sale, whichever is smaller.

I maintain the assumption of a linear cost of production. Thus the firm will not explicitly use inventories to smooth production over time. There is nothing in this assumption that would bias the firm toward production counter-smoothing, though. A positive discount rate ensures that the firm will try to match production and sales. The model implies that in the limit as the uncertainty about demand is eliminated production identically equals sales.

If backlogging is feasible, the firm's total demand may include its excess demand from previous periods. A backlog occurs, say in period t, when in the event of a stockout a buyer gives the firm an option to sell the product in period $t+1$ at the period t price.[5] I assume that the firm can backlog some known portion of its excess demand in any period; this includes as special cases Abel's (1985) assumption of no backlogs, and West's (1983, Ch. 3) assumption of 100 percent backlogs. The cost to the firm of backlogging is the interest lost on the price of the goods, since money is assumed to change hands only at the time of the sale.

The assumptions regarding the timing and availability of production are worth emphasizing because they differ somewhat from those of other authors (for example, West, 1983; Abel, 1985). I assume that the production resulting from decisions based on period $t-1$ information is available for sale in period t. There are several possible interpretations of this assumption. One is that the production decision based on period $t-1$ information is implemented in period t, but quickly enough that the production is available for sale in period $t-1$ as well. A second interpretation is that the decision is implemented in period t, with the production available for period t. Still a third is that production and sales activity take place in alternate sub-periods, night and day, for example, with each night's production based on information including the day's sales. The convention for this paper will be the first, so the production indexed by t will always be based on information as of $t-1$, but available for sale in period t. West and Abel both assume a longer lag between the receipt of information and the availability of production for sale, namely that the production based on period $t-1$ information is not available for sale until period $t+1$. Which assumption is better is an empirical question; the answer certainly depends on the length of the decision period, and may differ from one industry to another. I would argue that this paper's assumption is better for production-to-stock industries. Industries with lengthy production periods (for example, airplane manufacturing, construction) tend to produce "to order" rather than to stock, and therefore do not hold finished goods inventories.

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[3] Thus the firm is in a so-called "production to stock" industry (see, for example, Moses Abramovitz, 1950, or Kenneth West, 1983). The monopoly assumption is made only for analytical convenience. Similar qualitative behavior occurs in perfect competition, but it is more difficult to obtain analytical solutions in that case (see my paper, 1986, Ch. 2).

[4] One might argue that stockouts cannot be very important because we never see them (that is, zero inventories) in the data. First, the optimal stockout probability derived in the paper can be arbitrarily close to zero. Second, we only observe inventories aggregated across goods and at points in time, so even if the stockout probability is high we would not expect to see zero inventories. Extension of the model to a firm that produces multiple products is reasonably straightforward, and the qualitative features of the one-good model remain.

[5] Readers might notice two shortcomings of this treatment of backlogs. First, a better assumption might be that the price the backlogged customer pays is the minimum of the current and past price. In this model, though, the two prices turn out to be the same. Second, in practice the firm's option may last more than one period. Here again, however, it will turn out that the firm will never want to carry over unfilled orders a second period.

The firm's demand in period t has a stochastic component representing new demand for that period, and a certain component representing backlogs from earlier periods. Prices enter the stochastic component multiplicatively, so that the firm's total demand in period t can be expressed as

$$x_t = h(p_t)S_t + b_{t-1}, \tag{21}$$

where h is a nonstochastic function of current period price p_t, S_t is an exogenous stationary stochastic process, and b_{t-1} is the stock of backlogs. Shocks to the demand process are multiplicative, that is, $S_t = E_{t-1}(S_t)u_t$, where the u_t are independently and identically distributed random variables with a mean of one.

If the firm does stock out in period t, then its excess demand is just $x_t - (n_{t-1} + y_t) = x_t - z_t$. I assume that the firm can backlog a portion q of its excess demand, where q is a known constant, so that

$$b_t = q(x_t - z_t). \tag{22}$$

The assumption that q is a known constant is restrictive, but does generalize the assumptions made by West (1983) (who assumed $q=1$), and Abel (1985) ($q=0$).

I assume that the only direct costs facing the firm are a linear cost of production, cy_t, and some constant opportunity cost of its resources, which implies a discount factor $\beta<1$. Inclusion of other linear costs—such as a direct inventory-carrying cost or stockout cost—is a straightforward extension of the model and does not affect the qualitative behavior of the firm. The firm's realized profit in period t, under the assumption that backorders sell at the previous period's price, is

$$\pi_t = p_t(z_t - b_{t-1}) + p_{t-1}b_{t-1} - cy_t. \tag{23}$$

Inventory carried into the next period satisfies the identity

$$n_t = n_{t-1} + y_t - z_t, \tag{24}$$

and sales at a given price can be no greater than the minimum of the available stock and demand:

$$z_t \leq \min[n_{t-1} + y_t, x_t]. \tag{25}$$

In what follows, I will assume that (25) holds with equality, that is, that the firm never turns down a sale. This will be justified below in the case of multiplicative demand shocks, since the firm will earn a constant positive markup in each period.

At the beginning of each period the firm chooses price and production to maximize the present discounted value of expected profits

$$E_{t-1}\left\{\sum_{s=t}^{\infty}\beta^{s-t}[p_s(z_s - b_{s-1}) + p_{s-1}b_{s-1} - cy_s]\right\}$$

subject to the inventory-production-sales identity (24), the nonnegativity constraint on inventories (25), the demand function (21), and the backlog equation (22). This expression for profits incorporates the assumption that the current sales to backlogged customers are made at the previous period's price.

To summarize: Each period the firm's demand consists of its backlogged sales from previous periods, about which it is certain, plus new demand, which it does not know at the time it makes its production decision. In choosing its level of production, the firm must weigh the possibility that it will stock out against the possibility that it will end up with inventory to be carried over into the next period. The key assumption is that the firm cannot literally sell a good that it does not have in stock.

The firm's profit-maximization problem can be solved using standard dynamic programming techniques. Suppose $E_t(S_{t+1})$ is a known function f of a vector of state variables Γ_t. For example, Γ_t might include current and lagged sales if demand is serially correlated, but does not include lagged or current values of production or price. To keep matters reasonably simple I will assume that randomness in Γ_t is through u_t (which would be the case if $\log(S_t)$ were some arbitrary ARMA process), but this is not necessary for the results that follow. The fundamental dynamic programming equation is given by

$$(26)\quad \begin{aligned} &J(n_{t-1}, b_{t-1}, p_{t-1}, \Gamma_{t-1}) \\ &\quad = \max_{p_t, y_t} E_{t-1}\{ p_t(z_t - b_{t-1}) + p_{t-1}b_{t-1} \\ &\qquad\qquad - cy_t + \beta J(n_t, b_t, p_t, \Gamma_t)\}, \end{aligned}$$

subject to the constraints (21), (22), (24), and (25).

Let $G(\ )$ denote the cumulative distribution function (assumed to be continuous and strictly monotonic) for the forecast error u_t. Also, let $Q_t \equiv [n_{t-1} + y_t - b_{t-1}]/[h(p_t)f(\Gamma_{t-1})]$, the ratio of the amount the firm makes available for sale (in excess of backlogs) to expected nonbacklog demand. Then (26) can be written as

$$(26')\quad \begin{aligned} &J(n_{t-1}, b_{t-1}, p_{t-1}, \Gamma_{t-1}) \\ &\quad = \max_{p_t, y_t} \Big\{ p_t h(p_t) f(\Gamma_{t-1}) \int_0^{Q_t} u_t \, dG(u_t) \\ &\qquad + p_t h(p_t) f(\Gamma_{t-1}) \int_{Q_t}^{\infty} Q_t \, dG(u_t) \\ &\qquad + p_{t-1}b_{t-1} - cy_t \\ &\qquad + \beta \Big[\int_0^{Q_t} J[h(p_t)f(\Gamma_{t-1}) \\ &\qquad\qquad \times (Q_t - u_t), \\ &\qquad\qquad 0, p_t, \Gamma_t] \, dG(u_t) \\ &\qquad\quad + \int_{Q_t}^{\infty} J[0, qh(p_t)f(\Gamma_{t-1}) \\ &\qquad\qquad \times (u_t - Q_t), \\ &\qquad\qquad p_t, \Gamma_t] \, dG(u_t) \Big] \Big\}. \end{aligned}$$

The first line of the maximand in (26′) is expected profits in period t expressed as a function of the control variables y_t and p_t (note that Q_t is a function of y_t). The second and third lines are the discounted value function, also expressed as a function of y_t and p_t. A stockout occurs if the forecast error u_t is greater than Q_t, so the probability that the firm stocks out in period t is $1 - G(Q_t)$. The first-order condition for the production decision is:[6]

$$(27)\quad \begin{aligned} 0 = (p_t - c)(1 - \beta q)[1 - G(Q_t)] \\ - c(1 - \beta)G(Q_t). \end{aligned}$$

This condition can be interpreted as follows: At a given level of production, the firm will stock out with probability $1 - G$, in which case it loses $p - c$ in the current period, while backlogging a portion q of the excess demand. Thus the net gain to the firm from producing another unit this period, conditional on stocking out, is $(p - c)(1 - \beta q)$. With probability G, however, the firm will not stock out, in which case an increase in production this period is just a shift from the next period. Hence conditional on not stocking out the firm loses $c(1 - \beta)$ as a result of producing an extra unit this period. At the optimum the net expected gain from producing another unit should equal zero. It is easy to verify that at the optimum $G(Q_t)$ will lie strictly between 0 and 1 provided $p_t > c$ and $0 \leq \beta < 1$. Note that except for the change of variables (Q_t instead of A_t), necessitated by multiplicative rather than additive uncertainty, the conditions (3) and (13) from the example of the previous section are special cases of (27) with, respectively, $q = 0$ and $q = 1$.

Equation (27), or something very close to it, holds much more generally than just under the assumption in this paper. With a more general specification of uncertainty, G

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[6] The simplest way to derive the first-order condition is to solve the maximization period with a finite horizon T, starting at period T and working backward, with an arbitrary concave value function for the state variables at time T. It then becomes clear that the solution is stationary in the sense that decisions prior to $T - 1$ do not depend on the period T-value function.

would be replaced by the distribution function for demand; a more general cost structure would allow parameters such as c, β, and q to be functions of time and of the control variables y and p. But for given values of p, c, β, q, and for a given distribution of demand, (27) must hold at the optimum. It expresses the tradeoff between stocking out and carrying over inventory as described earlier. Note, for example, that a larger markup implies, *ceteris paribus*, a lower stockout probability (more production), while a greater ability to backlog increases the firm's optimal stockout probability.

The first-order condition with respect to price is a little more complicated. Letting $\underline{u}(Q_t) \equiv E(u_t|u_t < Q_t)$, and $e_t \equiv -(\partial h/\partial p_t)(p_t/h)$ (the price elasticity of expected demand), we have

$$\begin{aligned}(28)\quad 0 = {} & \underline{u}G + Q_t(1-G) \\ & + \beta q\left[1-\left(\underline{u}G + Q_t(1-G)\right)\right] \\ & - e_t\left\{\frac{p_t - \beta c}{p_t}\right\}\underline{u}G \\ & + \beta q\left\{\frac{p_t - c}{p_t}\right\}\left[1-\underline{u}G\right].\end{aligned}$$

This condition is perfectly analogous to the standard monopolist's first-order condition for price. An increase in price raises revenue on existing sales, but drives some customers away. The positive terms in (28) equal expected sales (expressed as a proportion of expected demand), including those sales that are backlogged, that is, not executed until the following period. The negative term is proportional to the elasticity of expected demand; it takes into account that some customers will in expectation be lost as a result of the price increase. The primary difference between (28) and the corresponding condition under certainty ($(p-c)/p = 1/e$) is the possibility of no reduction in sales in response to a price increase, which arises because of the positive stockout probability. In other words, the elasticity of expected sales will generally be less than the elasticity of expected demand, because with probability $1-G$ the firm will stock out, in which case there will be no change in sales, on the margin, in response to a price increase. This intuition suggests that the markup will be greater than under certainty, and indeed this is the case. Using (27) to simplify (28) yields the following nonclosed-form expression for the markup

$$(29)\quad \frac{p_t - c}{p} = \frac{1}{e_t}\left[\frac{\left[\underline{u}G + Q(1-G)\right](1-\beta q) + \beta q}{\underline{u}(1-\beta q) + \beta q}\right].$$

The factor multiplying $1/e$ is greater than one because $Q_t > \underline{u}$. This result extends the similar finding of Samuel Karlin and Charles Carr (1962) to the case where the monopolist can backlog its excess demand.[7]

To see how price behaves over time, note that equation (27) implies that Q_t depends only on p_t, β, c, q, and on the distribution of u_t. Also, the elasticity depends only on p_t. Therefore, no state variables enter the right side of (29), and we get the result that price is constant over time. This greatly simplifies the solution to the problem of optimal production, but the results in Section III regarding production variability are not sensitive to the assumptions that lead to a constant price. For example, they carry over to the case of a competitive equilibrium in which price adjusts *ex post* to clear the market (see my dissertation, 1986, Ch. 2). Under competition the firm wishes to avoid stockouts because sales are marginally profitable at the market-clearing price in the event of a stockout (that is, the nonnegativity constraint is binding).

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[7] See the related work of Edward Zabel (1970, 1972), who also finds that multiplicative uncertainty leads to a higher price than under certainty. Blinder (1982) considers the pricing behavior of inventory-carrying firms facing various kinds of additive uncertainty. Equation (29) makes clear that, as with the textbook monopolist, some restrictions on $h(p_t)$ will generally be necessary for an optimum to exist. For the present purposes, it will suffice to assume that at any price the elasticity of expected demand is finite and bounded from below by a number greater than one. This will ensure that the markup is positive but finite, and that the firm makes nonnegative expected profits.

The intuition for the constant price result is simply that the markup depends on the price elasticity of expected sales, which is constant, not on the expected level of sales. A firm with, for example, an unexpectedly large stock of inventories does better by saving on current production costs and selling entirely out of inventory than by cutting its price and maintaining its level of production. Although with nonmultiplicative uncertainty, firms would generally respond to changes in expected demand by changing their prices, the variance of production and the variance of sales would, to a first approximation, be affected the same way. Procyclical price changes, for example, would smooth both production and sales. Hence a more general specification of uncertainty would be unlikely to overturn the results presented in Section III.

The constancy of the profit-maximizing price immediately implies that the firm chooses production so that the probability of stocking out is the same every period. To see this, note that equation (27) expresses Q_t as an implicit function of p, c, q, β, and the distribution function G, all of which are constant. Therefore, the stockout probability $1-G(Q_t)$ is constant.

We can now completely characterize the decision rule of the firm in terms of the state variables of the problem. In what follows I will let $\bar{x}_{t-1}$ denote $E_{t-1}(h(p)S_t)$, the firm's expectation of *new* demand (demand exclusive of backlogs) in period t conditional on information at $t-1$. Substituting for Q_t in (27), and defining the parameter

$$k = G^{-1}\left[\frac{p-c}{p-\dfrac{\beta c(1-q)}{1-\beta q}}\right]$$

(the optimal choice of Q_t), we have

$$n_{t-1} + y_t = b_{t-1} + k\bar{x}_{t-1}. \tag{30}$$

Equation (30) shows that the firm makes available for sale in period t an amount equal to its backlogged sales b_{t-1} plus a multiple k of its expected new demand. One can think of k as the derivative of production with respect to expected demand. It can theoretically take on any value in the support of the error distribution. The technical reason for this multiplier effect is that firms choose production with regard to stockout probabilities, not first moments. In general, the difference between the means of two probability distributions will not equal the difference between the quantiles (that is, the points at which the distribution function is equal to some given value). Therefore, unless the optimal stockout probability happens to be such that k equals one, a change in expected demand should lead to something other than a one-for-one change in expected production. The results of Section III concerning production counter-smoothing, however, hold for *any* value of $k > 0$.

We can now solve for production and sales in terms of demand expectations and innovations. Equation (30) and the identity $y_t = z_{t-1} + a_t - a_{t-1}$ together imply

$$y_t = z_{t-1} + k(\bar{x}_{t-1} - \bar{x}_{t-2}) + b_{t-1} - b_{t-2}. \tag{31}$$

Thus production is equal to the most recent period's sales, plus the change in the stock of backlogged sales, plus a factor proportional to the change in expected new demand. We can immediately see that if no backlogging were possible ($q = 0$), *and* if demand were independently and identically distributed, then production would just equal lagged sales, so the variances of production and sales would be equal. The ability of this model to account for the greater variability of production, then, requires relaxing at least one of those restrictions.

Equations (25) and (30) together imply the following expression for sales:

$$z_t = b_{t-1} + \bar{x}_{t-1}v_t, \tag{32}$$

where $v_t \equiv \min[k, u_t]$. Equation (32) says that actual sales equal the stock of backlogs, plus new demand or the remaining stock,

TABLE 1—SIMULATION RESULTS[a]

	$q=0$				$q=1$			
			var(y)/var(z)				var(y)/var(z)	
e	p/c	k	$\rho=0.1$	$\rho=0.5$	p/c	k	$\rho=0.1$	$\rho=0.5$
1.1	11.11	1.998	1.622	2.990	11.09	1.820	1.613	2.773
1.5	3.03	1.990	1.619	2.978	3.01	1.336	1.834	2.270
2	2.02	1.980	1.615	2.963	2.01	1.002	1.767	1.816
4	1.35	1.944	1.603	2.914	1.33	0.501	1.280	1.238
10	1.12	1.847	1.577	2.793	1.11	0.200	1.050	1.039
50	1.03	1.447	1.530	2.467	1.02	0.040	1.002	1.001

[a] The discount factor used in the above calculations was 0.99. Variance ratios come from Monte Carlo simulations with a sample size of 1500.

whichever is smaller. A stockout occurs if $u_t > k$.

Combining the above expressions for production and sales with the specification of backlogs (22), we get the following equations for production, sales, backlogs, and inventories:

$$(33)\quad y_t = k(\bar{x}_{t-1} - \bar{x}_{t-2}) + \bar{x}_{t-2}v_{t-1} + q\bar{x}_{t-2}(u_{t-1} - v_{t-1})$$

$$(34)\quad z_t = \bar{x}_{t-1}v_t + q\bar{x}_{t-2}(u_{t-1} - v_{t-1})$$

$$(35)\quad b_t = q\bar{x}_{t-1}(u_t - v_t)$$

$$(36)\quad n_t = \bar{x}_{t-1}(k - v_t).$$

These expressions can be readily understood if one bears in mind the relationships between u, v, and k. If $u < k$, then $v = u$; otherwise $v = k$. In the first case, backlogs are zero, leftover inventories are positive, and sales are equal to total demand. In the second, leftover inventories are zero, backlogs positive, and sales are constrained by the available stock.[8]

Example: Suppose $\bar{x}_t = p_t^{-e}[\bar{x}_{t-1}^{\rho}u_t]$, and let u_t be distributed uniformly over the interval $[0,2]$, so that $G(u) = (1/2)u$. It turns out that the markup satisfies a quadratic equation involving the discount rate β, the elasticity of demand e, and the backlog factor q.

Table 1 presents analytically calculated values for the markup (defined as p/c) and k, and estimates of the ratio of the variance of production to the variance of sales based on simulation results, for various values of the elasticity e and the serial correlation parameter ρ. Note that the ability to backlog has, perhaps not surprisingly, a dramatic effect on inventory levels and hence on production volatility. Without backlogs k is substantially greater than one even for very small markups, whereas with backlogs k is small at even fairly substantial markups. In the cases in which, for example, $k > 1$ (that is, the stockout probability is less than one-half), the ratio of var(y) to var(z) ranges from 1.53 to 2.99. These values are as large or larger than the empirical ratios documented in Blinder (1986).

The next section considers the question of whether inventory behavior as derived above is likely to lead to production countersmoothing.

III. The Variance of Production

A number of authors have suggested that a target inventory motive might account for the fact that the variance of production typically exceeds the variance of sales. The purpose of this section is to determine sufficient conditions for production counter-smoothing to occur. To understand why some sort of target motive is a logical candidate to explain the "excess" variance of production, consider the identity $y_t = z_{t-1} + a_t - a_{t-1}$. Thinking of a_t as the target and y_t as the control variable, this has the behavioral interpretation that production must make up for the previous period's sales (that is, replenish the inventory stock) *and* respond to changes in the desired stock available for sale. The corresponding variance relationship is

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[8] It is possible for (33) to yield a negative solution for production, either because the first term (which reflects changes in expectations) can be arbitrarily negative in a given period, or because of an exogenously given large initial inventory stock. Technically one can rule out the former by bounding the support of the forecast error distribution. The second situation is inherently transitory; for example, in the case of independently and identically distributed demand the stochastic steady state will always have $y_t > 0$.

$$(37)\qquad \operatorname{var}(y) = \operatorname{var}(z) + \operatorname{var}(\Delta a) + 2\operatorname{cov}(z_{-1}, \Delta a).$$

In any model in which the firm's desired level of a_t depends on expected future sales, the variance of production will exceed the variance of sales unless changes in expected future sales are negatively correlated with the most recent period's sales.

Even so Abel (1985) is able to show in a model that incorporates the nonnegativity constraint on inventories that profit-maximizing firms may *smooth* production even if their costs are linear. Abel's model has a longer production lag (as described earlier) and independently and identically distributed demand; with a production lag as in this paper and constant marginal cost, it yields equal variances of production and sales. Taken together, Abel's results and those in this paper suggest that adding serial correlation or backlogs to demand makes production more variable, while lengthening the production lag tends to smooth production relative to sales.

We have seen already that with independently and identically distributed demand and no backlogging, the two variances are identical, since production equals lagged sales. The first result of this section shows that backlogs alone induce production counter-smoothing.

PROPOSITION 1: *If demand is independently and identically distributed and $q > 0$, then the variance of production exceeds the variance of sales.*

PROOF:

With independently and identically distributed demand, equations (33) and (34) simplify to

$$(38)\qquad y_t = [v_{t-1} + q(u_{t-1} - v_{t-1})]\bar{x},$$

and

$$(39)\qquad z_t = [v_t + q(u_{t-1} - v_{t-1})]\underline{x},$$

where $\underline{x}$ is the (constant) expectation of new demand. Since u and v are serially uncorrelated, it follows that

$$(40)\qquad \operatorname{var}(y) = \operatorname{var}(z) + 2q\bar{x}^2\operatorname{cov}(v, u - v).$$

But v and $u - v$ are nondecreasing functions of u, so the covariance term is positive.

The intuition for Proposition 1 is that backlogs shift sales away from periods of large unexpected demand, while production responds to the previous period's excess demand. This is easiest to see in the case $q = 1$. Production each period equals lagged sales plus the change in the stock of backlogs. Recalling that each period's backlogs are always filled, we then have that production equals last period's sales to new customers, plus the current stock of backlogs (since $z_{t-1} + (b_{t-1} - b_{t-2}) = (z_{t-1} - b_{t-2}) + b_{t-1}$). Backlogs, though, are the difference between the last period's demand and sales, if positive; hence production equals lagged new demand (which is clear from equation (8) above with $q = 1$), and the variance of production equals the variance of new demand.[9] The time series of sales, on the other hand, will be smoother than that of new demand, since sales will be shifted away from periods with large positive demand shocks.

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[9] Another way to understand this case is that with $q > 0$, the firm can act more like a "production-to-order" firm (see West, 1983), so that its production will be more backward looking. The model with $q = 1$ is not, however, a model of a production-to-order industry, since some output is produced to stock. It is perhaps best thought of as an industry in which firms can backlog, but competitive pressures force some degree of production to stock.

Now suppose that $q=0$, but that demand is not independently and identically distributed. The next result shows that the production counter-smoothing result found in Section I holds in this more general setting in which the firm chooses price optimally.

PROPOSITION 2: *If* $q=0$ *and demand is not independently and identically distributed (in the sense that* $\bar{x}_{t-1}$ *is not a constant), and if demand expectations are positively related to current sales, then the variance of production exceeds the variance of sales.*

PROOF:

With $q=0$ we have

$$y_t = z_{t-1} + k\Delta\bar{x}_{t-1},$$

so

$$\operatorname{Var}(y) = \operatorname{Var}(z) + k^2 \operatorname{var}(\Delta\bar{x}_{t-1}) + 2k\operatorname{cov}(z_{t-1}, \Delta\bar{x}_{t-1}).$$

If $\bar{x}_{t-1}$ depends positively on z_{t-1}, then $\operatorname{cov}(z_{t-1}, \Delta\bar{x}_{t-1})$ will be positive, which also implies $\operatorname{var}(y) > \operatorname{var}(z)$.

Thus changes in expectations must be negatively correlated with sales for production to be smoother than sales, even without backlogs. This result holds for any $k>0$, so counter-smoothing may still occur even if production responds much less than one-for-one with changes in expectations.

The intuition for Proposition 2 is the same as in the case considered in Section I. A demand shock has two effects: it changes the end-of-period inventory stock by one-for-one, and it changes expectations about future demand. If demand is positively serially correlated, the two effects work in the same direction. A positive shock, for example, lowers inventories (thereby increasing optimal production one-for-one), and increases expected demand for the following period. The latter effect also increases optimal production, so the total innovation in production is larger than the innovation in sales. Thus both serial correlation in demand and the ability to backlog have the effect of increasing the variance of production relative to the variance of sales.[10]

IV. Conclusions

This paper has presented a model of inventory behavior that incorporates stockouts, backlogs, and serially correlated demand. The behavior of optimal production under plausible assumptions about the environment was shown to imply that the variance of production will exceed the variance of sales even in the absence of movements in productivity or costs. Hence the observation of production counter-smoothing does not bear on the question of the relative importance of demand shocks versus productivity shocks.

With strictly convex costs it is likely that optimal production would be smoothed relative to the predictions of the model, but a simple continuity arguments implies that strict convexity of the cost function is consistent with production counter-smoothing. Moreover, a number of other factors that are left out of the formulation could work in the direction of further increasing the volatility of production. Changes in interest rates will generally affect both the demand for the durable goods and the cost of holding inventories. An increase in the interest rate, for example, would lower both expected demand and desired inventory accumulation. Thus to the extent that interest rates affect demand even greater volatility of production might occur.

One question that arises is the relationship between this model and the linear-quadratic specification as in Blanchard (1983) and West (1986). First, the "target" inventory behavior that emerges in this paper is similar to the *ad hoc* specification employed in those models, except that here current period production is included in the beginning-of-period inventory stock. Thus for the linear-quadratic target specification to have more of a claim for being an approximation to an optimal policy, its target inventory component should be modified accordingly. The second difference is the convex costs of production and of changing production in the linear-quadratic model. Both of the above-mentioned authors imposed a strict quadratic specification of production cost (e.g., $c(y) = hy^2$). If those models were estimated with linear and quadratic cost terms (which would be useful anyway, given West's excess volatility result), along with the modified target inventory specification, then there might be some basis for the claim that the specification approximates the objective of a firm with general convex costs and a nonnegativity constraint on inventories, especially if the quadratic coefficients turn out to be small or insignificant. Conversely, a test for the significance of the quadratic coefficients could be a valid test of the constant marginal cost assumption employed in this paper.

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[10] Backlogs and serial correlation together do not interact perversely to overturn the results of Propositions 1 and 2. For example, it is straightforward to show that if $q=1$, then the variance of production exceeds the variance of *demand* (which itself is more variable than sales), provided demand exhibits positive serial correlation.

It would be useful to specify the demand side of the model in greater detail. The general equilibrium results alluded to earlier (my paper, 1986, Ch. 2) suggest that the results of this paper are robust to more rigorous treatments of demand. That model assumes time-separable utility though, which is not very realistic in the case of consumer durables. A more complete treatment of intertemporal substitution on the part of consumers would be a useful extension, especially in the model with backlogs.

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The Information in Long-Maturity Forward Rates

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Current 1-year forward rates on 1- to 5-year U.S. Treasury bonds are information about the current term structure of 1-year expected returns on the bonds, and forward rates track variation through time in 1-year expected returns. More interesting, 1-year forward rates forecast changes in the 1-year interest rate 2- to 4-years ahead, and forecast power increases with the forecast horizon. We attribute this forecast power to a mean-reverting tendency in the 1-year interest rate.

Much of the empirical work on the term structure of interest rates is concerned with two questions. (a) Do current forward rates forecast future interest rates? (b) Do current forward rates have information about the structure of current expected returns on bonds with different maturities? Much of the empirical work on these questions uses U.S. Treasury bills and so is restricted to maturities less than a year. This paper studies the information in forward rates about future interest rates and current expected returns for annual U.S. Treasury maturities to 5 years.

Our results on expected bond returns are novel. Past tests typically fail to produce reliable inferences about the structure of expected returns for maturities beyond a year. (See, for example, Reuben Kessel, 1965; J. Huston McCulloch, 1975; and Fama, 1984b.) Using the regression approach in Fama (1984a, 1986), we are able to infer that 1-year expected returns for maturities to 5 years, measured net of the interest rate on a 1-year bond, vary through time. These expected premiums swing from positive to negative, however. On average, the term structure of 1-year expected returns on 1- to 5-year Treasury bonds is flat.

Differences in expected returns are usually interpreted as rewards for risk. In this view, our evidence that the ordering of expected returns across maturities changes through time implies changes in the ordering of risks. This behavior of expected returns is inconsistent with simple term structure models, like the liquidity preference hypothesis of John Hicks (1946) in which expected returns always increase with maturity. The evidence poses an interesting challenge to models like those of Robert Merton (1973), John Long (1974), Douglas Breeden (1979), and John Cox et al. (1985), that allow time-varying expected returns.

Our results on the forecast power of forward rates are also novel. Previous tests find little evidence that forward rates can forecast future interest rates. For example, Michael Hamburger and E. N. Platt (1975) and Robert Shiller et al. (1983) conclude that forward rates have no forecast power. Fama (1984a) finds some power to forecast 1-month interest rates 1 month ahead. We confirm that forward rate forecasts of near-term changes in interest rates are poor. When the forecast horizon is extended, however, forecast power increases. The 1-year forward rate calculated from the prices of 4- and 5-year bonds explains 48 percent of the variance of the change in the 1-year interest rate 4 years ahead. We argue that this forecast power is largely due to a slow mean-reverting tendency in interest rates which is more apparent over longer horizons.

The hypothesis that interest rates are mean reverting is prominent in old and new models of the term structure, for example, F. A. Lutz (1940) and Cox et al. Unlike other recent work (for example, Charles Nelson and Charles Plosser, 1982, and Fama and Michael Gibbons, 1984), our results offer supporting evidence.

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I. Regression Tests: Theory

Treasury bonds with maturities longer than a year are not issued on a regular basis, and only irregularly spaced maturities are available. To estimate a term structure for regularly spaced maturities, some method of interpolation must be used. We use such a method (see the Appendix) to construct end-of-month prices for 1- to 5-year discount bonds. From the prices, we calculate forward rates, returns, and interest rates for annual maturities to 5 years.

The tests of the information in forward rates about current expected returns and future interest rates are simple regressions of future returns and changes in interest rates on current forward rates. As in most term structure work, however, even simple tests require a tedious notation.

A. *Definitions of Variables*

The return on an x-year discount bond bought at time t and sold at $t+x-y$, when it has y years to maturity, is defined as

$$(1)\quad h(x,y:t+x-y) = \ln p(y:t+x-y) - \ln p(x:t),$$

where ln indicates a natural log, and $p(x:t)$ is the price of the bond at t. Symbols before a colon are the maturities that define a variable. The symbol after the colon is the time the variable is observed. Since most of the empirical variables are annual, time is measured in annual increments. For example, $h(5,4:t+1)$ is the 1-year return from t to $t+1$ on a 5-year bond.

The yield $r(x:t)$ on a discount bond with \$1 face value and x years to maturity at t is defined as

$$(2)\qquad r(x:t) = -\ln p(x:t).$$

The yield $r(1:t)$ on a 1-year bond is called the 1-year spot rate. It has a prominent role in the tests.

The time t 1-year forward rate for the year from $t+x-1$ to $t+x$ is

$$(3)\quad f(x,x-1:t) = \ln p(x-1:t) - \ln p(x:t) = r(x:t) - r(x-1:t).$$

For example, $f(5,4:t)$ is the forward rate for the year from $t+4$ to $t+5$.

The time t price of an x-year discount bond that pays \$1 at maturity is the present value of the \$1 payoff discounted at the time t expected values (E_t) of the future 1-year returns on the bond,

$$(4)\quad p(x:t) = \exp[-E_t h(x,x-1:t+1) - E_t h(x-1,x-2:t+2) - \ldots - E_t r(1:t+x-1)].$$

Equation (4) is a tautology, implied by the definition of returns. It acquires testable content when we add the hypothesis that the expected returns in (4) are rational forecasts used by the market to set $p(x:t)$. Equation (4) then says that the price contains rational forecasts of equilibrium expected returns. This hypothesis about the price is the basis of the tests.

B. *Forward Rates and Future Spot Rates*

For example, the forward rate $f(x,x-1:t)$ can be viewed as the rate set at t on a contract to purchase a 1-year bond at $t+x-1$. Motivated by this view, the literature has long been concerned with the hypothesis that the forward rate rationally forecasts the 1-year spot rate, $r(1:t+x-1)$, to be observed at $t+x-1$. To focus on the forecast of the spot rate in $f(x,x-1:t)$, we sum the first $x-1$ expected returns in (4) and write the price as

$$(5)\quad p(x:t) = \exp[-E_t h(x,1:t+x-1) - E_t r(1:t+x-1)].$$

Substituting (5) into (3) and subtracting the 1-year spot rate $r(1:t)$ gives

$$(6)\quad f(x,x-1:t)-r(1:t) = [E_t r(1:t+x-1)-r(1:t)] + [E_t h(x,1:t+x-1)-r(x-1:t)].$$

We call $f(x,x-1:t)-r(1:t)$ the forward-spot spread. Our tests of the information in the forward rate $f(x,x-1:t)$ about the future spot rate $r(1:t+x-1)$ then center on the slope in the forecasting regression,

$$(7)\quad r(1:t+x-1)-r(1:t) = a_1+b_1[f(x,x-1:t)-r(1:t)] + u_1(t+x-1).$$

Evidence that b_1 is greater than 0.0 implies that the forward-spot spread observed at time t has power to forecast the change in the 1-year spot rate $x-1$ years ahead.

Equation (6) holds for realized returns as well as expected values,

$$(8)\quad f(x,x-1:t)-r(1:t) = [r(1:t+x-1)-r(1:t)] + [h(x,1:t+x-1)-r(x-1:t)].$$

It follows that the regression (7) is complementary to the regression

$$(9)\quad h(x,1:t+x-1)-r(x-1:t) = -a_1+(1-b_1)[f(x,x-1:t)-r(1:t)] - u_1(t+x-1).$$

As indicated, the intercepts in (7) and (9) sum to 0.0, the residuals sum to 0.0 every period, and, most interesting, the slopes sum to 1.0. Thus, the slope in (7) estimates the split of variation in the forward-spot spread between the two terms of (6): (i) the forecasted change in the 1-year spot rate from t to $t+x-1$; and (ii) the premium of the $(x-1)$-year expected return on an x-year bond over the time t yield on an $(x-1)$-year bond.

Since b_1 is a constant, the estimated split of variation in the forward-spot spread does not change through time. This means that (7) can tell us that the forward-spot spread has power to forecast the change in the spot rate, but the regression fitted values only track all variation in the forecasts when the expected change in the spot rate and the expected premium in (6) always vary in fixed proportion. This is a limitation of (7) and of similar regressions outlined below.

Expression (6) for the forward-spot spread combines expected returns in (4) to focus on information in the time t price, $p(x:t)$, about the return, $h(x,1:t+x-1)$, and the spot rate, $r(1:t+x-1)$, to be observed at the beginning of the last year in the life of the x-year bond. We turn now to a different grouping of the expected returns in (4) which focuses on the information in $p(x:t)$ about the 1-year return, $h(x,x-1:t+1)$, and the yield, $r(x-1:t+1)$, to be observed in 1 year.

C. *Forward Rates and 1-Year Expected Returns*

If we sum the last $x-1$ expected returns in (4), the price of an x-year bond is

$$(10)\quad p(x:t)=\exp[-E_t h(x,x-1:t+1) - E_t r(x-1:t+1)].$$

Substituting (10) into (3) and subtracting the spot rate $r(1:t)$ gives

$$(11)\quad f(x,x-1:t)-r(1:t) = [E_t h(x,x-1:t+1)-r(1:t)] + [E_t r(x-1:t+1)-r(x-1:t)].$$

Thus, when (10) is used for the price of an x-year bond, the forward-spot spread contains $E_t(x,x-1:t+1)-r(1:t)$, the time t expected premium of the 1-year return on an x-year bond over the 1-year spot rate. But the forward-spot spread also contains the

TABLE 1—TERM PREMIUM REGRESSIONS: ESTIMATES OF (12): 1964–85
$h(x, x-1: t+1) - r(1:t) = a_2 + b_2[f(x, x-1:t) - r(1:t)] + u_2(t+1)$

						Residual Autos (Yearly Lag)				
Dependent	a_2	$s(a)$	b_2	$s(b)$	R^2	1	2	3	4	5
$h(2,1:t+1)-r(1:t)$	−.21	.41	.91	.28	.14	−.01	−.12	−.07	−.17	−.01
$h(3,2:t+1)-r(1:t)$	−.51	.68	1.13	.37	.11	−.18	−.12	.03	−.17	−.05
$h(4,3:t+1)-r(1:t)$	−.91	.92	1.42	.45	.11	−.23	−.10	.02	−.14	−.08
$h(5,4:t+1)-r(1:t)$	−1.06	1.31	.93	.53	.05	−.17	−.11	.03	−.17	−.10

Note: $r(1:t)$ is the 1-year spot rate observed at t; $h(x, x-1:t+1)$ is the 1-year return (t to $t+1$) on an x-year bond, and $h(x, x-1:t+1) - r(1:t)$ is the term premium in the 1-year return. $f(x, x-1:t)$ is a 1-year forward rate observed at t, and $f(x, x-1:t) - r(1:t)$ is the forward-spot spread. The regression estimates the expected value of the term premium to be observed at $t+1$, conditional on the forward-spot spread observed at t. The standard errors, $s(a)$ and $s(b)$, of the regression coefficients are adjusted for possible heteroscedasticity and for the autocorrelation induced by the overlap of monthly observations on annual returns. (See Halbert White, 1980, and Lars Peter Hansen, 1982.) The regression R^2 is adjusted for degrees of freedom. The sample size is 252, corresponding to the period January 1964 to December 1984 for the forward-spot spreads and January 1965 to December 1985 for the term premiums. If the true autocorrelations are 0.0, the standard error of the estimated residual autocorrelations is about 0.065. The data are derived from the U.S. Government Bond File of the Center for Research in Security Prices (CRSP) of the University of Chicago. See the Appendix.

expected change from t to $t+1$ in the yield on $(x-1)$-year bonds. If yields are random walks, the expected yield change in (10) is 0.0, and $f(x, x-1:t) - r(1:t)$ is $E_t h(x, x-1:t+1) - r(1:t)$. The common finding that forward rates have little power to forecast interest rates suggests a world where yields are close to random walks.

We call $h(x, x-1:t+1) - r(1:t)$ the term premium in the 1-year return on an x-year bond. Our tests of the information in time t forward rates about time t 1-year expected returns then center on the slope in the regression,

$$(12)\quad h(x, x-1:t+1) - r(1:t) = a_2 + b_2[f(x, x-1:t) - r(1:t)] + u_2(t+1).$$

Evidence that b_2 is positive implies that the expected value of the term premium varies through time. Moreover, like (6), (11) holds for the realized returns, $h(x, x-1:t+1)$ and $r(x-1:t+1)$, as well as for their expected values. It follows that $1-b_2$ in (12) is the slope in the complementary regression,

$$(13)\quad r(x-1:t+1) - r(x-1:t) = -a_2 + (1-b_2)[f(x, x-1:t) - r(1:t)] - u_2(t+1).$$

Evidence that b_2 differs from 1.0 means that the forward-spot spread forecasts the change in the $(x-1)$-year yield 1 year ahead.

In short, the slope in (12) splits variation in the forward-spot spread between the 1-year expected term premium and expected yield change in (11), just as the slope in (7) splits variation in the forward-spot spread between the two multiyear forecasts in (6).

II. Expected Term Premiums

A. *Time-Varying Expected Term Premiums*

Estimates of the term-premium regression (12) are in Table 1. Three slopes are more than 3.0 standard errors from 0.0, and the fourth is 1.75 standard errors from 0.0. We infer that expected term premiums in 1-year returns for maturities to 5 years vary through time, and so are typically nonzero. The results are in contrast to previous work of Kessel, McCulloch, Fama (1984b), and others that finds no convincing evidence of incremental expected returns for maturities beyond a year. The tests extend to longer maturities the conclusion of Richard Startz (1982), Shiller et al., and Fama (1984a, 1986), that expected bill returns contain time-varying maturity premiums. The evidence also confirms Shiller's (1979) claim that if bond prices are rational, the high variability of

TABLE 2—AUTOCORRELATIONS, MEANS, AND STANDARD DEVIATIONS: 1964-85

			Autocorrelations (Monthly Lag)										
Variable	$\bar{x}$	$s(x)$	1	2	3	4	5	6	12	24	36	48	60
Spot and Forward Rates: $r(1:t)$ and $f(x, x-1:t)$													
$r(1:t)$	7.49	2.90	.97	.93	.89	.86	.84	.81	.71	.48	.31	.15	.09
$f(2,1:t)$	7.59	2.81	.97	.94	.92	.89	.87	.86	.75	.59	.43	.24	.12
$f(3,2:t)$	7.74	2.69	.96	.94	.92	.90	.89	.87	.75	.59	.41	.23	.11
$f(4,3;t)$	7.80	2.79	.96	.94	.92	.91	.89	.87	.75	.59	.44	.24	.11
$f(5,4:t)$	7.74	2.65	.93	.90	.89	.88	.87	.85	.75	.60	.41	.26	.12
Term Premiums: $h(x, x-1:t+1)-r(1:t)$													
$h(2,1:t+1)-r(1:t)$	−.11	2.18	.90	.80	.72	.65	.59	.53	.08	−.10	−.16	−.24	−.05
$h(3,2:t+1)-r(1:t)$	−.23	3.75	.89	.79	.70	.63	.56	.49	−.01	−.09	−.11	−.22	−.09
$h(4,3:t+1)-r(1:t)$	−.46	5.13	.89	.80	.71	.64	.57	.48	−.04	−.08	−.09	−.21	−.10
$h(5,4:t+1)-r(1:t)$	−.83	6.36	.89	.80	.73	.66	.58	.49	−.04	−.07	−.08	−.22	−.13
Forward-spot spreads: $f(x, x-1:t)-r(1:t)$													
$f(2,1:t)-r(1:t)$	.10	.91	.82	.78	.71	.64	.59	.53	.34	−.12	−.42	−.22	.03
$f(3,2:t)-r(1:t)$	.26	1.13	.78	.66	.57	.51	.48	.45	.29	−.14	−.39	−.23	.01
$f(4,3:t)-r(1:t)$	.31	1.22	.74	.66	.62	.53	.51	.48	.33	−.10	−.39	−.25	−.03
$f(5,4:t)-r(1:t)$	.25	1.54	.77	.62	.53	.52	.53	.47	.32	−.14	−.34	−.23	−.09
Residuals from Spot Rate Autoregression (14)													
$e(t)$	.00	.69	.15	−.08	−.12	−.11	.08	−.08	−.07	.06	−.07	−.02	−.04

Note: $r(1:t)$ is the 1-year spot rate observed at t; $h(x, x-1:t+1)$ is the 1-year return (t to $t+1$) on an x-year bond; $f(x, x-1:t)$ is a 1-year forward rate observed at t; $e(t)$ is the residual from the first-order autoregression (14) fit to the monthly time series of $r(1:t)$. The sample size is 252, corresponding to the period January 1964 to December 1984 for the forward-spot spreads and the period January 1965 to December 1985 for the term premiums in 1-year returns. If the true autocorrelations are 0.0, the standard error of the estimated autocorrelations is about 0.065. The data are derived from the CRSP U.S. Government Bond File. See the Appendix.

yields on longer-term bonds implies time-varying expected returns.

The slopes b_2 in the term-premium regressions range from 0.91 to 1.42. All are within one standard error of 1.0. We can infer that the slopes (equal to $1-b_2$ and with the same standard errors as b_2) in the complementary yield-change regression (13) are within one standard error of 0.0. The results suggest that when forward-spot spreads are viewed as in (11), variation in current spreads is mostly variation in the term premiums in current 1-year expected returns, and forward-spot spreads do not predict yield changes 1 year ahead. The evidence extends to longer maturities Fama's (1984a, 1986) conclusion for bills that forward rates are close to current expected returns.

B. *The Behavior of Expected Term Premiums*

Kessel, McCulloch, Fama (1984b), and others show that inferences from average returns about average expected returns on longer-maturity bonds are imprecise because of the high variability of returns. An alternative approach is to use the term-premium regressions as a license to infer average expected returns from average forward rates. The average forward rates in Table 2 show no strong tendency to increase or decrease across longer maturities. The average value of the 4- to 5-year forward rate, $f(5,4:t)$, only exceeds the average value of the 1-year spot rate, $r(1:t)$, by 0.25 percent per year.

However, the structure of forward rates varies through time, and the picture provided by average forward rates is misleading. Figure 1 plots the 5-year forward-spot spread. General patterns of variation are similar for other maturities. If forward-spot spreads are expected term premiums in 1-year returns, Figure 1 shows the general path of the variation through time of expected term premiums. The forward-spot spread is characterized by alternating runs of positive and negative values. At least after 1970, there seems to be a relation between the sign of

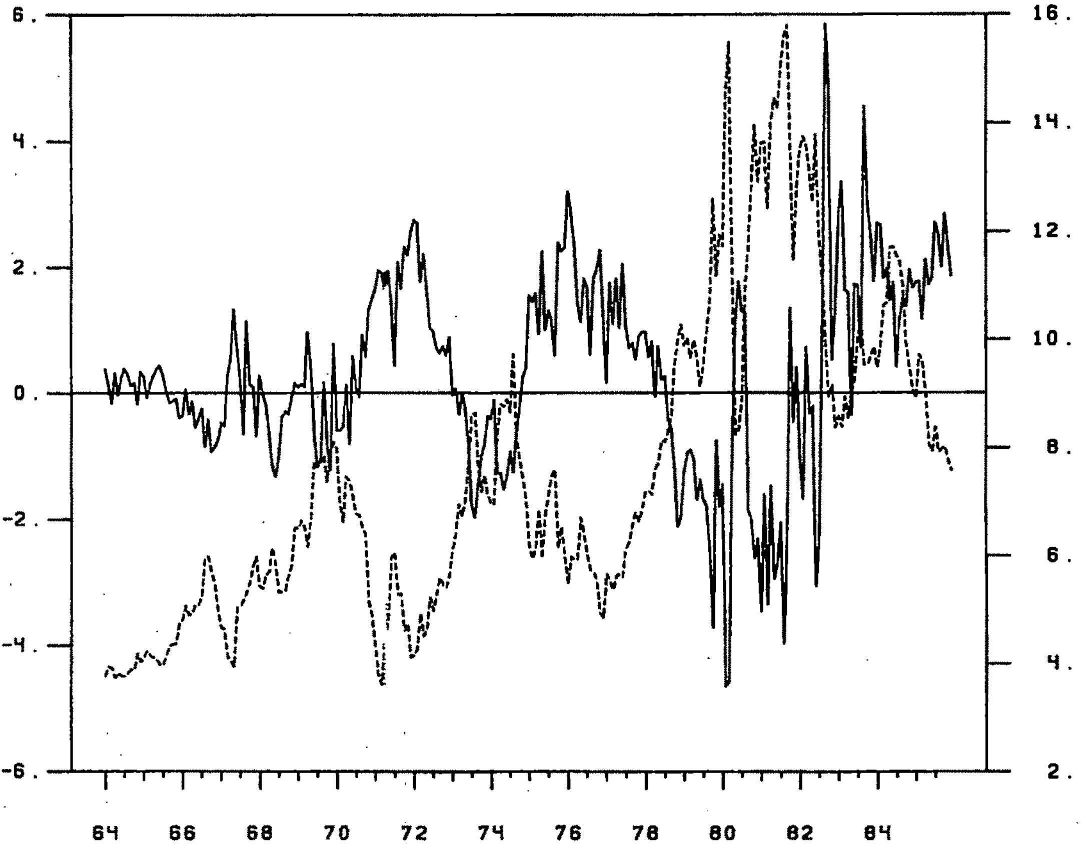

FIGURE 1. SPOT RATE AND 5-YEAR FORWARD-SPOT SPREAD

Note: 5-year forward-spot spread, $f(5,4:t)-r(1:t)$, (solid line) and the 1-year spot rate, $r(1:t)$, (dashed line). The left vertical axis is the scale (percent per year) for the spread, and the right is the scale for the spot rate. The horizontal axis is the date t.

the forward-spot spread and the business cycle. Positive forward-spot spreads in Figure 1 tend to be associated with periods of strong business activity, for example, 1970–72, 1975–78, 1983–85. Negative spreads tend to occur during the recessions of 1973–74 and 1979–82.

In short, the average forward rates in Table 2 suggest that the term structure of expected 1-year returns on 1- to 5-year Treasury bonds is on average flat. But Figure 1 shows that a flat term structure of forward rates is not typical. The path of the forward-spot spread in Figure 1 suggests that expected term premiums are typically nonzero and vary between positive and negative values. Such changes in the ordering of expected returns, and their apparent relation to the business cycle, pose an interesting challenge to term structure models that can accommodate time-varying expected returns.

C. *The Effects of Measurement Error*

A caveat about the term-premium regressions is in order. The spot rate $r(1:t)$ is obtained from a 1-year bill, but the implied prices of discount bonds used to estimate forward rates and returns for longer maturities involve interpolation that can produce measurement error. Errors in the long-maturity price $p(x:t)$ in $f(x,x-1:t)$ tend to bias the slope in the term-premium regression (12) toward 1.0, since $p(x:t)$ is the

TABLE 3—REGRESSION FORECASTS OF THE CHANGE IN THE SPOT RATE

								Residual Autos (Yearly Lag)				
Dependent	a	$s(a)$	b_1	$s(b_1)$	b_2	$s(b_2)$	R^2	1	2	3	4	5
$\mathbf{r(1:t+x-1)-r(1:t)=a+b_1[f(x,x-1;t)-r(1:t)]+u(t+x-1)}$												
$r(1:t+1)-r(1:t)$	.21	.41	.09	.28			.00	−.11	−.12	−.07	−.17	−.01
$(1:t+2)-r(1:t)$	.40	.73	.69	.26			.08	.39	−.21	−.36	−.33	−.08
$r(1:t+3)-r(1:t)$	.57	.75	1.30	.10			.24	.52	.04	−.40	−.46	−.25
$r(1:t+4)-r(1:t)$	1.12	.61	1.61	.34			.48	.38	.09	−.09	−.23	−.27
$\mathbf{r(1:t+x-1)-r(1:t)=a+b_2[[\hat{r}(1:t+x-1)-r(1:t)]+u(t+x-1)}$												
$r(1:t+1)-r(1:t)$	.03	.44			.87	.40	.16	−.01	−.05	−.02	−.12	.03
$r(1:t+2)-r(1:t)$	.16	.70			.88	.20	.26	.43	−.08	−.15	−.10	.04
$r(1:t+3)-r(1:t)$	.22	.84			.90	.13	.33	.57	.16	−.15	−.14	−.04
$r(1:t+4)-r(1:t)$	.37	.80			.91	.21	.36	.58	.29	.03	−.18	−.10
$\mathbf{r(1:t+x-1)-r(1:t)=a+b_1[f(x,x-1:t)-r(1:t)]+b_2[\hat{r}(1:t+x-1)-r(1:t)]+u(t+x-1)}$												
$r(1:t+1)-r(1:t)$	.04	.43	−.14	.24	.90	.39	.16	−.03	−.04	−.00	−.09	.05
$r(1:t+2)-r(1:t)$	.15	.74	.19	.30	.82	.17	.26	.44	−.07	−.17	−.15	.01
$r(1:t+3)-r(1:t)$	.22	.84	.76	.43	.69	.22	.40	.56	.19	−.15	−.27	−.17
$r(1:t+4)-r(1:t)$	.79	.79	1.21	.45	.38	.20	.51	.47	.19	.03	−.14	−.23

Note: $r(1:t)$ is the 1-year spot rate observed at t; $f(x,x-1:t)$ is a 1-year forward rate observed at t; $\hat{r}(1:t+x-1)$ is the time t forecast of $r(1:t+x-1)$ from the first-order autoregression (14) fit to the time-series of $r(1:t)$. The standard errors of the regression coefficients are adjusted for possible heteroscedasticity and for the autocorrelation induced by the overlap of monthly observations on annual or multiyear changes in the spot rate. See White and Hansen. The sample size in the regressions for the 1-year change in the spot rate, $r(1:t+1)-r(1:t)$ is 252, corresponding to the period January 1964 to December 1984 for the forward-spot spread and the period January 1965 to December 1985 for the 1-year changes in the 1-year spot rate. An additional year (12 months) of data is lost each time the forecast horizon $t+x-1$ is extended an additional year. Thus, the regressions for the 4-year change, $r(1:t+4)-r(1:t)$, have 216 observations. Under the hypothesis that the true autocorrelations are 0.0, the standard error of the estimated residual autocorrelations is about 0.065. The data are derived from the CRSP U.S. Government Bond File. See the Appendix.

purchase price for the return $h(x,x-1:t+1)$. Errors in the short-maturity price $p(x-1:t)$ in $f(x,x-1:t)$ tend to bias the slope in (12) toward 0.0 since $-\ln p(x-1:t)$ is the yield $r(x-1:t)$ in the complementary yield change regression (13). The net effect of measurement error on the slopes in the term-premium regressions is thus difficult to predict.

It is easier to predict the effect of measurement error on the spot-rate forecasting regression (7). The future spot rate $r(1:t+x-1)$ is calculated from the price of a 1-year bill at $t+x-1$. Measurement errors in the time t prices in $f(x,x-1:t)$ tend to bias the slope in (7) toward 0.0 and so attenuate our ability to identify forecast power in the forward rate. The forecast power we find in the tests that follow is thus in spite of any bias due to measurement error in forward rates.

III. Forecasts of 1-Year Spot Rates

A. *Estimates of the Spot-Rate Forecasting Regression*

Since slopes close to 1.0 in the estimates of the term-premium regression (12) suggest that forward rates do not forecast yields 1 year ahead, intuition suggests that they will not forecast longer-term changes in rates. Intuition is not confirmed by the slope estimates for regression (7) in Table 3. The slopes are more than 2.6 standard errors from 0.0 for all forecasts beyond a year. The forward-spot spread, $f(x,x-1:t)-r(1:t)$, forecasts the change in the 1-year spot rate, $r(1:t+x-1)-r(1:t)$, 2 to 4 years ahead. Moreover, forecast power improves with the forecast horizon: $f(3,2:t)-r(1:t)$ explains 8 percent of the variance of the change in the spot rate 2 years ahead; $f(4,3:t)-$

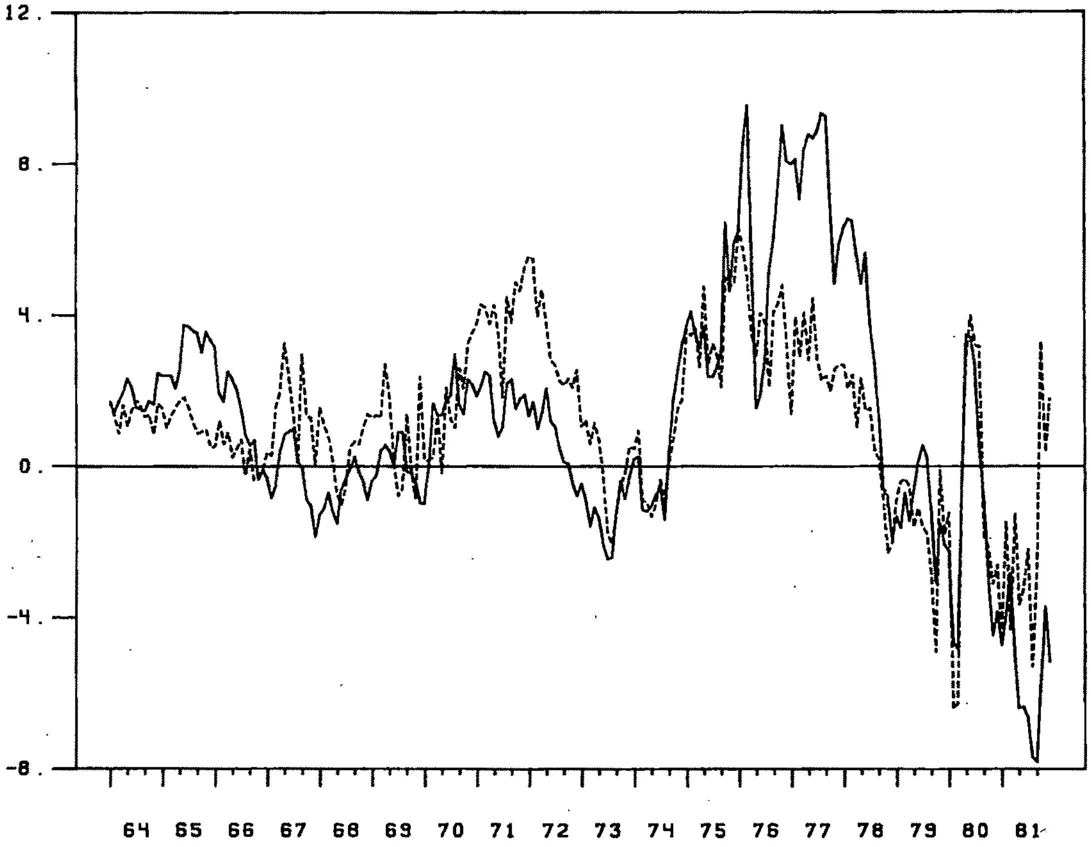

FIGURE 2. 4-YEAR CHANGE IN SPOT RATE AND FORECAST FROM REGRESSION (7)

Note: 4-year change in the spot rate, $r(1:t+4)-r(1:t)$, (solid line) and the forecasted change (dashed line) from the regression (7) of $r(1:t+4)-r(1:t)$ on the forward-spot spread $f(5,4:t)-r(1:t)$. The vertical axis is percent per year and the horizontal axis is t, the date of the forecast.

$r(1:t)$ explains 24 percent of the variance of the 3-year change; $f(5,4:t)-r(1:t)$ explains 48 percent of the variance of the 4-year change.

Figure 2 plots 4-year changes in the 1-year spot rate and the fitted values from the regression of $r(1:t+4)-r(1:t)$ on $f(5,4:t)-r(1:t)$. The figure suggests that the high R^2 (0.48) of the regression reflects consistent long-term forecast power during the sample period. We argue that long-term forecast power is largely due to slow mean reversion of the spot rate.

B. *Forecast Power and Mean Reversion*

The autocorrelations of $r(1:t)$ in Table 2 are close to 1.0 at short lags, but they decay across longer lags. The pattern suggests that month-to-month levels of the 1-year spot rate are highly autocorrelated, but the spot rate has a slow mean-reverting tendency.

The Appendix shows that if the 1-year spot rate is mean-reverting (stationary), then for long forecast horizons the expected change in the spot rate due to mean reversion explains half the variance of the actual change. If the spot rate is slowly mean reverting, the expected change explains more of the variance of the change for longer forecast horizons. Recall that the proportions of the variance of the change in the 1-year spot rate explained by forward-spot spreads in the estimates of (7) increase with the forecast horizon and reach 0.48 for 4-year changes. Thus, the forecast power of forward-spot spreads conforms to what is predicted by slow mean reversion.

It is also easy to show that if the spot rate is highly autocorrelated but slowly mean reverting, the correlation of the nonoverlapping changes, $r(1:t+T)-r(1:t)$ and $r(1:t)-r(1:t-T)$, is close to 0.0 for small values of T but approaches -0.5 for large values of T. The correlations between nonoverlapping 1-, 2-, 3-, and 4-year changes in the 1-year spot rate are -0.13, -0.29, -0.51, and -0.52.

B. *Forecast Power and Mean Reversion: Direct Evidence*

Although slow mean reversion of the spot rate is sufficient to explain the long-term forecast power of forward rates, the pattern of decay of the autocorrelations of the spot rate suggests that a first-order autoregression (AR1) is a reasonable model for the 1964–85 period. Fitting an AR1 to monthly observations on the 1-year spot rate yields

$$(14)\quad r(1:t) = \underset{(.121)}{.257} + \underset{(.015)}{.968}\,r(1:t-1)+e(t),$$

where the numbers in parentheses are standard errors. The autocorrelations of the residuals (Table 2) are generally close to 0.0. The first-order residual autocorrelation, 0.15, suggests that the model can be improved by adding a (1-month) moving-average term. Since we are interested in forecasts of spot-rate changes over long horizons, and since our goal is to document a mean-reverting tendency in a simple way, we stick with the AR1.

We use (14), fit to monthly levels of the 1-year spot rate, to forecast changes 1 to 4 years ahead. Regressions of actual on forecasted changes test for forecast power due to slow mean reversion. If the spot rate has a slow mean-reverting tendency which is well-approximated by the estimated AR1, the regressions of changes on the changes forecast by the AR1 will have forecast power that increases with the forecast horizon. Note, however, that the estimated AR1 slope, 0.968, is barely 2.0 standard errors from 1.0. If the spot rate is actually a random walk, the regressions of changes on the changes forecast by the estimated AR1 will have no power.

Regression intercepts (Table 3) that are all within 1.0 standard error of 0.0 and slopes within 1.0 standard error of 1.0 suggest that the AR1 forecasts of spot-rate changes are unbiased. As predicted by slow mean reversion, the forecasts explain more of the variance of spot-rate changes for longer forecast horizons. Explained variance rises from 16 percent for 1-year changes to 36 percent for 4-year changes. However, variance explained never reaches 50 percent, the limit predicted by mean reversion. An AR1 may not be the best model for the spot rate. But the AR1 serves its purpose. Its power to forecast spot-rate changes is consistent with a tendency toward mean reversion of the spot rate.

C. *Forward Rate vs. AR1 Forecasts*

Is the forecast power of forward rates solely due to mean reversion of the spot rate? Multiple regressions (Table 3) of spot-rate changes on forward-spot spreads and the AR1 forecasts are a test. In the regressions for 1- and 2-year changes, the slopes for the forward-spot spread are close to 0.0, while the slopes for the AR1 forecasts are close to 1.0 and more than 2.0 standard errors from 0.0. In the multiple regressions for 3- and 4-year changes, both the forward-spot spreads and the AR1 forecasts have marginal explanatory power (slopes almost or more than 2.0 standard errors from 0.0).

The fact that the AR1 dominates forward-spot spreads in the 1- and 2-year regressions does not necessarily mean that the market misses some of the mean reversion of the spot rate. We know from (6) that $f(x,x-1:t)-r(1:t)$ contains both the spot-rate forecast, $E_t r(1:t+x-1)-r(1:t)$, and the premium, $E_t h(x,1:t+x-1)-r(x-1:t)$. If the expected change in the spot rate is a varying proportion of the forward-spot spread, the fixed-coefficient regression of $r(1:t+x-1)-r(1:t)$ on $f(x,x-1:t)-r(1:t)$ will not track all the information in the forward rate about the future spot rate.

There is evidence that variation in forward rates is not due entirely to forecasts of a mean-reverting spot rate. Although forward-spot spreads move opposite the spot rate, the correlations of the spot rate with 2- to 5-year forward-spot spreads (-0.25, -0.37, -0.30, and -0.42) are far from -1.0. If forward rates were driven only by mean reversion of the spot rate, our longest maturity forward rate, $f(5,4:t)$, would be much less variable than the spot rate. Table 2 shows that the standard deviation of $f(5,4:t)$ is within 10 percent of that of $r(1:t)$.

On the other hand, the fact that forward-spot spreads have marginal explanatory power relative to the AR1 in 3- and 4-year regression forecasts may reflect deficiencies of the AR1. One possibility is that reversion to a constant mean is not a complete story for the spot rate. Such mean reversion predicts that forward-spot spreads are positive (the spot rate is expected to increase) when the spot rate is low and negative when the spot rate is high. The plots of the spot rate and the 5-year forward-spot spread in Figure 1 confirm this prediction for most of the sample period. However, some of the lowest values of the spot rate occur in 1964–68. Mean reversion predicts positive expected changes in spot rates for this period, but forward-spot spreads are not systematically positive. Likewise, when the spot rate drops in 1982, it remains above its sample mean. Simple mean reversion predicts that forward-spot spreads should not become positive, but they do.

Scott Ulman and John Wood (1983) suggest that the mean of the spot rate rises with the end of the gold standard in 1971 because a fiduciary currency means higher average inflation rates. A rise in the mean of the spot rate is consistent with the behavior of the spot rate and the forward-spot spread in Figure 1. John Campbell and Shiller (1984) suggest a time-series model for the spot rate that includes mean reversion, but toward a mean that can change through time.

The important point for our purposes is that the Table 3 regressions are sufficient to conclude that much of the forecast power of forward rates is due to a mean-reverting tendency of the spot rate. But we cannot infer on the basis of our simple tests that the forecast power of forward rates is better or worse than expected on the basis of the mean reversion of the spot rate.

Finally, the autocorrelations of forward rates (Table 2) are close to those of the spot rate. The same slow decay is observed in autocorrelations of 1- to 5-year yields (not shown). A slow mean-reverting tendency is apparently a general property of interest rates—an appealing conclusion with strong roots in term structure theory. However, because the autocorrelations of interest rates are close to 1.0 for short lags, recent empirical studies (for example, Nelson-Plosser and Fama-Gibbons) tend to conclude that interest rates are approximately random walks. Our evidence that long-term changes in rates are predictable suggests that the slow decay of the autocorrelations should instead be emphasized.

V. Conclusions

A. *Forward Rates and the Term Structure of Expected Returns*

The estimates of the term-premium regression (12) allow us to infer that 1-year expected returns for U.S. Treasury maturities to 5 years, measured net of the interest rate on a 1-year bond, vary through time. Moreover, at least during the 1964–85 period, this variation of expected term premiums seems to be related to the business cycle. Expected term premiums are mostly positive during good times but mostly negative during recessions.

Differences in expected returns are usually interpreted as rewards for risk. Our evidence, like that for shorter maturities in Fama (1986), suggests that the ordering of risks and rewards changes with the business cycle. This behavior of expected returns is inconsistent with simple term structure models, like the liquidity preference hypothesis of Hicks and Kessel in which expected returns always increase with maturity. Perhaps it can be explained by models, like those of Merton, Long, Breeden, and Cox et al., that allow time-varying expected returns. The challenge is apparent.

B. *Forward Rates and Future Spot Rates*

Like earlier work, we find little evidence that forward rates can forecast near-term changes in interest rates. When the forecast horizon is extended, however, forecast power improves, and 1-year forward rates forecast changes in the 1-year spot rate 2 to 4 years ahead. We conclude that this forecast power reflects a slow mean-reverting tendency of interest rates.

Like any interest rate, the 1-year spot rate on Treasury bonds can be split into an expected real return and an expected inflation rate. The mean reversion of the spot rate likely implies that both of its components are mean reverting, a hypothesis with strong economic appeal. However, theory does not suggest that the processes that generate expected inflation and expected real returns should be the same, or that the processes should not change through time. We interpret the mean reversion of interest rates documented here as a tendency with details to be documented by future work.

APPENDIX

A. *Data*

The U.S. Government Bond File of the Center for Research in Security Prices (CRSP) has end-of-month data for all U.S. Treasury securities. We use the data to estimate end-of-month term structures for taxable, noncallable bonds for annual maturities to 5 years. The approach is outlined below. Our data are available to subscribers to the CRSP bond file.

Each month a term structure of 1-day continuously compounded forward rates is first calculated from available maturities. Bills are used for maturities to a year. To extend beyond a year, the pricing assumption is that the daily forward rate for the interval between successive maturities is the relevant discount rate for each day in the interval. Suppose daily forward rates for month t are calculated for maturities to T and the next bond matures at $T+k$. Coupons on the bond to be received prior to T are priced with the daily forward rates from t to each payment date. Coupons and the principal to be received after T are priced with the daily forward rates from t to T and with the (solved for) daily forward rate for T to $T+k$ that equates the price of the bond at t to the value of all payments. These calculations generate a step-function term structure in which 1-day forward rates are the same between successive maturities.

Prior to the recent large deficits, the number of noncallable, fully taxable bonds available each month is small. From 1964 onward there is at least one bond in each 1-year maturity interval to 5 years, but often there are few bonds beyond 5 years. We sum the daily forward rates to generate end-of-month term structures of yields for annual maturities to 5 years. The yields are used to calculate implied prices of 1- to 5-year discount bonds, from which we calculate the term-structure variables used in the tests.

B. *Forecast Power and Mean Reversion*

We use a first-order autoregression (AR1) to illustrate that predictable changes in the spot rate due to slow mean reversion are more apparent over longer horizons. Suppose $z(t)$ is an AR1 with parameter ϕ and mean μ,

$$\text{(A1)}\quad z(t)=\delta+\phi z(t-1)+\in(t),$$

$$\mu=\delta/(1-\phi).$$

The time t expected value of $z(t+T)$ is (see Nelson, 1973, p. 148)

$$\text{(A2)}\quad E_t z(t+T)=\mu+\phi^T[z(t)-\mu],$$

and the expected change in $z(t)$ from t to $t+T$ is

$$\text{(A3)}\quad E_t z(t+T)-z(t)$$

$$=[z(t)-\mu](\phi^T-1).$$

If ϕ is close to 1.0, the expected change in $z(t)$ is small for small values of T. The expected change in $z(t)$ increases with the forecast horizon and approaches $\mu - z(t)$: $z(t)$ is expected to revert to its mean μ.

The variance of the expected change in $z(t)$ is

$$\sigma^2[E_t z(t+T) - z(t)] = \sigma^2(z)(\phi^T - 1)^2, \tag{A4}$$

which grows with T and approaches $\sigma^2(z)$. How much of the variance of the T-period change is explained by the expected change? Since $\text{cov}[z(t+T), z(t)]$ approaches 0.0 for large values of T, the variance of $z(t+T) - z(t)$ approaches $2\sigma^2(z)$. Since $\sigma^2[E_t z(t+T) - z(t)]$ approaches $\sigma^2(z)$, for long forecast horizons the expected change in $z(t)$ due to the mean reversion of $z(t)$ explains half the variance of $z(t+T) - z(t)$. This result is a general property of stationary processes; it is not special to an AR1.

For an AR1 with ϕ close to 1.0, the ratio of the variance of the expected change to the variance of the change approaches 0.5 from below. For example, if $\phi = 0.95$, $\sigma^2[E_t z(t+1) - z(t)]$ is $0.025\sigma^2[z(t+1) - z(t)]$. Thus, the expected change explains more of the variance of the change for longer horizons. This is a rather general implication of slow mean reversion.

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Is the Distinction Between Anticipated and Unanticipated Money Growth Relevant in Explaining Aggregate Output?

By ROMAN FRYDMAN AND PETER RAPPOPORT*

The New Classical approach to macroeconomics emphasizes the need to distinguish between the effects on real output of rationally anticipated and unanticipated movements in nominal variables. This modeling imperative displaced a generation of "Keynesian" macroeconometric models that had paid scant attention to expectational phenomena, and to the way in which expectations are formed.

An important expression of the anticipated-unanticipated distinction is the hypothesis that anticipated nominal changes have no effect on real output. The first empirical tests of this hypothesis were carried out by Robert Barro (1977, 1978), who found support for the neutrality hypothesis in the United States, using annual data. Several later investigators rejected anticipated policy neutrality. In particular, using quarterly data, Frederic Mishkin (1982; 1983, ch. 6) discovered that the hypothesis was rejected when the lag distributions of the nominal variables were substantially lengthened. While the consensus appears to be that this hypothesis is at variance with empirical evidence, the primacy of the anticipated-unanticipated distinction has remained The hypothesis of rational expectations was also tested in this framework by Mishkin and by Leonardo Leiderman (1980) (who used Barro's data), and was found to be consistent with the observed evidence.

In this paper we test the hypothesis that the anticipated-unanticipated distinction is irrelevant in explaining real output, for which we use the acronym AUDI.[1] An examination of this hypothesis is relevant for two reasons. First, it formalizes the modeling imperative introduced by the New Classical approach, which would not be supported if the hypothesis were not rejected. Second, the difficulties of measuring rational expectations empirically cast doubt on the validity of the statistical inferences in the studies cited above (see our 1986a paper). It would thus be desirable to reevaluate the empirical evidence concerning the New Classical propositions using methods less vulnerable to these difficulties. As we shall show, AUDI offers a simple approach which is not hamstrung by the problems of mismeasurement.

Below, we examine the performance of AUDI in the framework used by Barro and Mark Rush (1980) and by Mishkin, in which real output is regressed on variables supposed to capture the natural rate of output, and on distributed lags of proxies for anticipated and unanticipated money growth. The model and estimation methods are described in Section I. Our central finding is that, when examined in this framework, the AUDI hypothesis receives substantial empirical support, and its performance is robust to changes in the specification of rational expectations and of the natural rate of output (Sections II and III). Thus, the version of the output equation that has been estimated in studies of the New Classical propositions appears to collapse into one in which money growth affects output directly, without the intermediation of (rational) expectational phenomena. In view of our results concerning AUDI, we tested the hypothesis that "raw" money growth matters for the short-run behavior of real output (Section II). This hypothesis was strongly supported by the data.

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[1] An early examination of this hypothesis was carried out by Rappoport (1981). Subsequent to writing this paper we discovered that some tests for the U.K. have been done by Charles Bean (1984), and that John Boschen (1985) has examined the hypothesis using money announcements data. Neither considered the relationship of the AUDI hypothesis to the issue of mismeasurement of rational expectations.

These findings also aid interpretation of earlier empirical results on the rational expectations and neutrality hypotheses, which do not yield interpretable information when rational expectations are measured incorrectly (see our 1986a paper). Since there is no empirical distinction between the effects on output of anticipated and unanticipated money growth, the "standard" equation evidently provides no information about expectations. This implies that the "cross-equation restrictions" imposed in tests of the rational expectations hypothesis do not exercise any binding constraints. It is thus not surprising that these restrictions are not rejected empirically.

In order to reject short-run neutrality, Mishkin found it necessary to appeal to long lag distributions (five years) of anticipated and unanticipated money growth. Both he and Barro found this hypothesis to be supported using a two-year lag. These ambiguous results are not present when the data are viewed through AUDI. The support found for AUDI and the unambiguous rejection of the neutrality of raw money growth suggest that (anticipated) money growth is not neutral.[2] Indeed, we find the strongest support for nonneutrality in models with short distributed lags (four years or less), which also have the most plausible shapes on statistical and economic grounds.

I. Econometric Issues

A. *The Model and Hypotheses*

We shall examine the relationship of money growth and aggregate output in the context of Barro's (1977) and Mishkin's (1982; 1983, ch. 6) models of these two variables. Both of these models have the following form:

$$(1)\qquad y_t=\beta(L)(x_t-x_t^e)+\theta(L)x_t^e+y_{nt}+u_t$$

$$(2)\qquad x_t=z_t\delta+\lambda_t,$$

where $\beta_L=\beta_0+\beta_1L+\cdots+\beta_mL^m$ and $\theta(L)$ is defined analogously. The variable y represents aggregate real output, y_n its natural rate, x the rate of growth of the money supply, and x_t^e the forecast of x_t at $t-1$. The term z_t is a list of regressors chosen by the investigator, u_t and λ_t are random error terms. Following Barro-Rush, we assume that[3]

$$(3)\qquad \rho(L)u_t=\xi_t$$

where $\rho(L)=1-\rho_1L-\rho_2L^2$ and the ξ_t are identical and independently distributed.

The hypothesis that the anticipated-unanticipated distinction is irrelevant for explaining output (AUDI) is

$$(4)\qquad \theta=\beta$$

where $\theta=(\theta_0,\dots,\theta_m)'$ and β is similarly defined. If AUDI is valid, output does not depend on agents' expectations of money growth, in the sense that equation (1) may be rewritten as

$$(5)\qquad y_t=\tau(L)x_t+y_{nt}+u_t,$$

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[2] Stephen Cecchetti (1986) also rejects anticipated policy neutrality using an instrumental variable method that is not vulnerable to mismeasurement of expectation. His method cannot distinguish between anticipated money growth and raw money growth, and his results are consistent within the findings of this paper, and with the interpretation of earlier results given here.

[3] Mishkin used a fourth-order autoregressive specification in (3). Since the last two lags were never significant, we use the second-order representation. This saves degrees of freedom in the estimation of models with long lags of anticipated and unanticipated components of money growth.

where $\tau(L)$ is a lag polynomial of the same order as $\beta(L)$ and $\theta(L)$. The test whether money matters for the behavior of output is

$$\tau = 0. \tag{6}$$

In order to estimate the model, it is necessary to introduce proxies for expectations and the natural rate of output. The econometric significance of the natural rate is that it is the means of accounting for the nonstationarity in output. Barro-Rush and Mishkin replace y_{n_t} by a linear time trend (as well as a vector of economic variables). Following Charles Nelson and Charles Plosser (1982) we call this the trend stationary (TS) method of detrending. Nelson and Plosser have argued that the TS method is not appropriate for most macroeconomic time-series. Instead they suggest that a lag of output be used to represent the nonstationary in output, which they call the difference stationary (DS) method of detrending. Both the TS and DS versions of the output equation will be estimated.

To facilitate a comparison with the literature, we assume that agents' expectations are rational. Following the standard practice, we take $z_t\delta$ in equation (2) as a measure of expectations. This is typically described as the assumption of rational expectations, although $z_t\delta$ is not an exact measurement of rational expectations when the agents' information set is larger than z_t. In order that the output equation can be rewritten correctly once this substitution is made, it is necessary to decompose rational expectations into measured and unmeasured parts.

Let rational expectations, x^{re}, be defined by

$$x_t^{re} = E[x_t|\psi_t] \tag{7}$$

where ψ_t represents the agents' information set. We assume, for the sake of concreteness, that ψ_t exhausts the information available at $t-1$ for forecasting x_t.[4] We also assume that z_t is contained in or spanned by elements of ψ_t.

The linear projection of x^{re} on the (smaller) information set used by the investigator yields

$$x_t^{re} = z_t\delta + v_t \tag{8}$$

where $v_t \equiv E[x_t|\psi_t] - z_t\delta$, and v and z are contemporaneously orthogonal by construction. v, which is not necessarily serially uncorrelated,[5] is the investigator's error in measuring rational expectations, since the "instrument" $z_t\delta = \hat{x}_t^{re}$ is used to proxy rational expectations. Substituting this expression in (1) yields

$$y_t = \beta(L)(x_t - \hat{x}_t^{re}) + \theta(L)\hat{x}_t^{re} + y_{n_t} + \eta_t, \tag{9}$$

or equivalently,

$$y_t = \beta(L)(x_t - z_t\delta) + \theta(L)(z_t\delta) + y_{n_t} + \eta_t \tag{10}$$

where

$$\eta_t = (\theta(L) - \beta(L))v_t + u_t. \tag{11}$$

Although equation (7) was derived under the assumption of rational expectations, the output equation has the same form if agents' expectations are not rational. All that is required is the redefinition of v_t in (8) as the investigator's error in measuring expectations (rather than rational expectations). Formally, we can rewrite (8) as

$$v_t \equiv x_t^e - z_t\delta \tag{12}$$

where x_t^e is some arbitrary expectations function. In contrast to the case of rational expectations, v_t defined by (12) is not, in general, orthogonal to z_t. However, this does not affect tests of the hypotheses described above. It is obvious that with this redefinition of v_t, the resulting output equation still has the form given by equation (10), and will collapse to (5) if AUDI is true.

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[4] The set ψ_t can contain, for example, "raw" variables, functions of variables and parameters, or products of dummy and stochastic variables. Thus, the characterization of rational expectations used here does not rule out nonlinear and other expectations mechanisms.

[5] For a detailed discussion of the properties of v and other econometric issues, see our 1986a paper.

B. *Estimation Methods and Test Statistics*

Equations (10) and (11) show that the standard method of modeling rational expectations allows the output equation to be estimated only at the cost of introducing into the error term a distributed lag of v_t. If $\theta \neq \beta$, v_t will be correlated with z_{t-i} for $i > 0$, and so standard estimators of β and θ will be inconsistent. Obviously, if AUDI is true, η_t reduces to u_t, and mismeasurement of expectations does not jeopardize the statistical properties of estimators of these parameters.[6]

In formulating appropriate estimators for the parameters of equation (1), the serial correlation in u_t has to be taken into account. In general, past values of u will influence the variables in z_t and so iterative methods such as generalized least squares will not yield consistent estimators. As a first step, we ρ-difference equation (1) to yield

$$y_t = \beta^*(L)(x_t - z_t\delta) + \theta^*(L)z_t\delta + \rho(L)y_{nt} - (\rho(L)-1)y_t + \eta_t^*, \tag{13}$$

where $\beta^*(L) = \rho(L)\beta(L)$, $\theta^*(L) = \rho(L)\theta(L)$, and $\eta_t^* = (\theta^*(L) - \beta^*(L))v_t + \xi_t$. We note that $\beta^*(L) = \theta^*(L)$ if and only if $\beta(L) = \theta(L)$, whence $\eta_t^* = \xi_t$. A test of $\beta^* = \theta^*$ is thus equivalent to a test of $\beta = \theta$. Under AUDI, consistent estimates of β^* and θ^* can be derived (conditional on a consistent estimate of δ) if the regressors in (13) are orthogonal to ξ_t. Sufficient conditions for this are that the variables in z_t that are not lags of m_t and y_t are orthogonal to u_{t+i} $i \geq 0$, and that λ_t and ξ_t are contemporaneously uncorrelated. The first of these follows from the projection argument of Thomas Sargent (1976), and is similar to the conditions required for the instrumental variables by method of moments estimators developed in Lars Peter Hansen and Sargent (1982), for example.[7] The second condition stems from the presence of contemporaneous unanticipated money growth in equation (1). A similar condition appears in Mishkin (1983).

Since test statistics are evaluated under the null hypothesis expectations figure nowhere in the model, and so the way they are measured does not affect the statistical properties of estimators and inference. This result will now be employed to motivate the simple forms of estimation method and test statistics that we use. We shall use a two-step method of estimation, in which (2) is run first to provide an estimated proxy for rational expectations, $\hat{x}_t^{re} = z_t\hat{\delta}$, where $\hat{\delta}$ is the OLS estimator of δ. The proxy $\hat{x}_t^{re}$ is then substituted for $\hat{x}_t^{re}$ in equation (13), and θ^* and β^* are then estimated yielding $\hat{\theta}^*$ and $\hat{\beta}^*$.[8]

One potential complication of the two-step method is that, in general, the estimated variance of $\hat{\theta}^*$ and $\hat{\beta}^*$ that is produced by OLS computer program packages must be corrected to allow for the fact that $\hat{\delta}$ rather than δ is used in the second stage. It turns out that this problem disappears when $\theta = \beta$, even in the presence of mismeasurement of rational expectations. Kevin Murphy and Robert Topel (1983, pp. 7 and 11) show that the standard estimator of the asymptotic covariance matrix of $(\hat{\beta}^*, \hat{\theta}^*)$ (for example, that provided by an OLS computer regression package) has to be altered by a factor that is a function of the sampling variance of $\hat{\delta}$ computed from the first-stage regression of (2). However, this factor is also a quadratic form in $\theta - \beta$. Since it is to be evaluated under the null hypothesis, in which $\theta - \beta = 0$, the correction factor disappears.[9,10]

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[6] Only when the power of the tests is considered (i.e., distributions are computed under the assumption $\theta \neq \beta$) does mismeasurement affect the results. Unfortunately, to calculate the power is a formidable task since in general, under the alternate hypothesis, the estimators of all overidentified parameters are inconsistent.

[7] An instrumental variable estimator appears to be infeasible because by construction the instruments cannot be found for the rationally unanticipated components.

[8] This method has been used by Barro (1977) and Barro-Rush.

We now turn to the efficiency of the two-step method under the assumption of AUDI. In examinations of the rational expectations or policy-neutrality hypotheses, the efficient method of estimation (when mismeasurement of rational expectations is not present) is a systems method such as FIML.[11] This uses information from both the output equation (13) and the money growth equation (2) to estimate the parameters of each.

Under AUDI, the two-step method is also efficient. It is convenient, in sketching this argument, to omit y_{nt} from the output equation. This does not affect the generality of the argument. The system to be estimated can be written as

$$y_t = \beta^*(L)(x_t - \hat{x}_t^{re}) + \theta^*(L)\hat{x}_t^{re} + \eta_t^*$$

$$x_t = z_t\delta + \lambda_t.$$

Let the $2(m+3)$ variables whose coefficients are β^* and θ^* be represented $B(\delta)$. Under the assumption that λ_t and η_t^* are uncorrelated (which amounts to λ_t and ξ_t being uncorrelated when AUDI is true), FIML amounts to minimization of

$$\lambda'\lambda\eta^{*\prime}\eta^*$$

(where $\lambda' = (\lambda_1, \ldots, \lambda_T)$, and η^* is similarly defined) with respect to β^*, θ^*, and δ. The covariance matrix of the FIML estimators (which are consistent under AUDI) is given by the inverse of the information matrix. The information matrix is block diagonal in δ and (β^*, θ^*) when evaluated at $\theta = \beta$. Hence, so is its inverse. The block corresponding to (β^*, θ^*) is

$$\sigma_{\eta^*}^{2}[B(\delta)'B(\delta)]^{-1}. \tag{14}$$

This is the formula for the correct covariance matrix of the two-step estimator of β^* and θ^* under AUDI (compare Murphy and Topel). Hence, we conclude that the two-step method is asymptotically efficient under AUDI. Furthermore, since the two-step estimators of $\sigma_{\eta^*}^2$ and δ are consistent under AUDI, a consistent estimator of (14) may be constructed from the parameters estimated in the two-step method.

Intuitively this result occurs because AUDI reduces (1) to (5), which does not contain expectations. Hence, under AUDI, the output and money growth equations are dislocated. Since their errors are assumed to be uncorrelated with each other, systems estimation results in statistics with the same properties as single-equation estimates.

II. Tests of AUDI using the Barro-Rush and Mishkin Specifications of the Output and Money Growth Equations

In this section we test the AUDI hypothesis in the context of the Barro-Rush and Mishkin specifications of the output equation. Both studies employed the trend stationary (TS) version of the natural rate of output, in that they included a constant and linear time trend. In addition, we include the Barro-Rush selective military draft and real federal government expenditures variables in our replication of their model.

To decompose money growth into anticipated and unanticipated components, Barro-Rush regress the money growth rate on a constant, six lagged values of the dependent variable, three lagged values of the logit of the unemployment rate, and the contemporaneous value of "FEDV," a measure of the deviation of federal government expenditure from its normal value. Mishkin's

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[9] See Murphy and Topel and Adrian Pagan (1984). Again, the treatment of this issue in the literature abstracts from the problem of mismeasurement of rational expectations.

[10] In analyses of the money-output equation system, it is customary to operate in terms of the asymptotic distributions of test statistics. While the asymptotic distributions of the three conventional statistics are identical under both the null and alternate hypotheses, we employ Wald statistics. In finite samples, when referred to a χ^2 distribution, these are less favorable to the null than the Lagrange multiplier or likelihood ratio statistics (see E. Berndt and N. E. Savin, 1977). In order to correct the potential overstatement of the size of the test we use the degrees of freedom adjustment suggested by G. B. A. Evans and Savin (1982).

[11] Systems methods are used by Leiderman and by Mishkin (1983).

TABLE 1—p VALUES FOR χ^2 TESTS OF $\beta_8 = \ldots \beta_m = \theta_8 = \ldots = \theta_m = 0$

Model	Number of Lags (m)					
	8	10	12	15	18	20
Barro-Rush (BR0)	.153	.457	.462	.062	.192	.116
Mishkin (M0)	.920	.489	.603	.281	.112	.310

Note: These tests are carried out on the "ρ-differenced" version of the output equation. Thus, the length of the lag of anticipated and unanticipated money growth terms actually used in estimation is $m^* = m + 2$. For example, when $m = 20$, the test that lags beyond the seventh are zero in the original equation is carried out by testing the restriction that lags 10 through 22 have zero coefficients in equation (13). The same procedure applies to the other tests in this paper.

regressors are a constant and four lagged values of each of the dependent variable, the average rate on Treasury bills and the high employment government surplus. We estimated those regressions over the sample period 1948:III–1976:IV.[12] Our results, which we do not exhibit to conserve space, replicated the coefficient estimates reported by Barro-Rush (p. 38) and Mishkin (1983, Table 6.A.20, cols. 1 and 4). We will call these models of money growth BR0 and M0, respectively.

The two studies also estimated the system for a number of different values of m, the length of the lag on anticipated and unanticipated money in equation (1). In particular, Mishkin found that the neutrality of anticipated policy was rejected in models with long lags, but not in short-lag models. For $m = 8$, 10, 12, 15, 18, and 20, we tested the hypothesis that lags of anticipated and unanticipated components of money growth beyond the seventh are zero:[13]

$$(15) \quad \beta_8 = \cdots = \beta_m = \theta_8 \cdots = \theta_m = 0.$$

The results of testing (15) are presented in Table 1. There is only one rejection at the 10 percent level, for the Barro-Rush model, when $m = 15$. Examination of the estimates of β and θ in the long-lag models reveals that, after becoming small at intermediate lags, they suddenly become larger in absolute value and have no tendency to die out at the tail end of the distribution. For example, in the Barro-Rush model, when $m = 15$, the "unscrambled" estimates of $\theta_{13}, \ldots, \theta_{15}$ are of the order of 1.5 in absolute value, which is approximately the size of the coefficients of the earlier lags.[14] This seems to indicate that the long-lag models are overparameterized, that is, they use up too many degrees of freedom.[15] The size of these

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[12] The starting date was chosen so that, in the version of the output equation for which $m = 20$ (the longest lag with which we experimented exhaustively), all anticipated variables would be "within-sample" predictions, rather than some being within sample and some lying outside the estimation interval for the money growth equation. The estimation interval for the output equation is 1954:I–1976:IV. This is the intersection of the sample periods used by Barro-Rush and Mishkin. Extension of the sample to 1979:IV yielded results qualitatively similar to those that follow. The data in the Barro and Rush data set are seasonally adjusted, and we used adjusted data for Mishkin's model. We are grateful to John Merrick for providing us with the data used by Barro-Rush. Additional data needed for Mishkin's model were taken from the Citibase data bank.

[13] We note that the test statistics for this hypothesis are valid only under the maintained hypothesis $\beta_0 = \theta_0, \ldots, \beta_7 = \theta_7$ since this combined with the null hypothesis eliminates $v_t, \ldots, v_{t-m}$ from the error term η_t in (11).

[14] Mishkin (1983, Table 6.A.15) exhibits estimates of a regression of output on twenty lags of only unanticipated money growth, in which the coefficients do not exhibit this property. We note that our equations contain both anticipated and unanticipated terms. When we reestimated them first using only unanticipated terms, then using only anticipated terms, and finally using only lags of money growth itself, we found lag distributions similar in shape to Mishkin's.

[15] For example, in the Mishkin model, when $m = 20$, the unconstrained equation has 92 observations and 50 regressors.

TABLE 2—p VALUES FOR χ^2 TESTS OF $\theta = \beta$ IN THE BARRO-RUSH AND MISHKIN MODELS

	Number of Lags (m)						
Model	7	8	10	12	15	18	20
Barro-Rush (BR0)	.625	.426	.459	.484	.032	.174	.073
Mishkin (M0)	.617	.863	.580	.654	.221	.105	.247

coefficients is also inconsistent with the macroeconomic "intuition" that the effects of changes in money growth on output last beyond three years ($m = 12$). In summary, it is hard to make a case for a model with long lags. Evidence on the performance of (15) in other models analyzed later in this paper also supports this view. However, for completeness we will present results for $m = 7$, 8, 10, 12, 15, 18, and 20.

Before we discuss the results for testing the AUDI hypothesis, it is important to note that the results for the Barro-Rush model must be regarded with extreme caution. The reason is that, as a result of the specification of the money growth equation, the design matrix in the output equation comes very close to being singular.[16]

With this caveat in mind, the results in Table 2 can be seen strongly to support the AUDI hypothesis. There is only one rejection at the 5 percent level, in the Barro-Rush model with $m = 15$. The hypothesis is never rejected at the 1 percent level. Moreover, the p values in the models with short lags ($m \leq 12$) are all in excess of 42 percent.[17] Thus, the empirical framework that has been used in the literature to address questions of policy neutrality and rationality of expectations delivers the result that the distinction between anticipated and unanticipated money is not statistically relevant in explaining the movements of aggregate output. We shall now demonstrate that this conclusion survives when the models are reestimated using alternative expectations functions.

TABLE 3—ALTERNATIVE MODELS OF MONEY GROWTH: p VALUES OF CHOW TESTS

Model	BR1	BR2	M1	M2
Against 0 Model	.016 (1)	.020 (3)	.013 (1)	.005 (3)
Against 1 Model		.142 (2)		.039 (2)

Note: BR for Barro-Rush; M for Mishkin. The degrees of freedom are shown in parentheses.

A. *Alternative Proxies for Expectations*

John Taylor (1983, p. 22) observed that the average money growth rate rose discretely during the early 1960's. Similarly, one might expect that the money growth process would be altered by the breakdown of the Bretton Woods system in the early 1970's (Willem Buiter, 1983, p. 223). If these changes in structure have in fact occurred, they should be incorporated into the proxy for rational expectations. Omitting them would contaminate the forecast errors in the money growth equations with systematic components.

In view of these considerations, we estimate two alternative money growth equations. In one specification, we permit the intercept to differ before and after 1963:III. In a second specification, we allow for this change in the constant term and also permit

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[16] For example, the contemporaneous value of anticipated money is an exact linear combination of six lags of the money growth rate, three lags of the logit of the unemployment rate and "FEDV". The money growth terms can be obtained from the lags of anticipated and unanticipated money in the output equation. The remaining variables are highly collinear with the lags of output and real government spending that appear in the output equation. As is well known, a slight perturbation of the design matrix can lead to large movements in coefficient estimates.

[17] As we discussed in Section I, we use here the "reduced-form" procedure. The applicability of the GLS procedure, used for example by Mishkin (1983), is contingent on the exogeneity of output with respect to money. Nevertheless, we ran our tests of AUDI using GLS and the results were qualitatively the same.

Table 4—p Values for χ^2 Tests of $\theta = \beta$ using Barro-Rush and Mishkin Output Equation and Alternative Models of Money Growth

Model	Expectations Proxy	Number of Lags (m) 7	8	10	12	15	18	20
Barro-Rush	BR1	.448	.359	.421	.376	.153	.028	.095
	BR2	.780	.472	.453	.523	.536	.021	.007
Mishkin	M1	.196	.356	.394	.175	.011	.010	.054
	M2	.447	.679	.540	.531	.092	.189	.259

the coefficients of the first and second lags of money growth to differ before and after 1971:III.

These models are a substantial improvement on those with no structural shifts, as Table 3 shows. The models are named according to the basic specification from which they originate (BR for Barro-Rush and M for Mishkin) and the number of structural breaks (0, 1, or 2). Chow tests of all the structural change models against the equations originally specified by Barro-Rush and Mishkin are highly significant. We also note that in Mishkin's equation the estimate of the constant term doubles after 1963:III while, after 1971:III, the effect of the first lag of money growth is halved and that of the second changes sign. Similar large shifts in the money growth terms are evidenced by the Barro-Rush equation. However, the change in the constant term, although significant, is only of the order of 10 percent in this equation.

The results of testing the AUDI hypothesis using these alternative models of expectations are reported in Table 4. We note that some low p values are evidenced by the long-lag models. There are two rejections at the 1 percent level and five at the 5 percent level. While the hypothesis in (15) is sometimes rejected at the 5 percent level in some of the long-lag models, the coefficients again evidence implausibly high values at the end of the long-lag distributions. The models incorporating lag distributions that are less than or equal to three years in length ($m \leq 12$), strongly support the AUDI hypothesis. This is especially the case for models using the money growth equations with two structural breaks (BR2 and M2), for which the p values are all in excess of 42 percent.

III. An Alternative Measure of the Natural Rate of Output

Up to this point we have followed the approach to detrending that is standard in the literature, that is, to use a deterministic time trend. Nelson and Plosser have argued that including a lagged value of the dependent variable is more appropriate for macroeconomic data than the TS method. In our paper (1986b) we presented empirical evidence suggesting this difference-stationary (DS) of detrending appears to be more appropriate in this context. We now proceed to discuss the results obtained from the DS model. The specifications of the variables are as described in Section II.

Our attempt to determine the length of the lag distribution of anticipated and unanticipated money growth yielded results similar to those found in the TS model. The hypothesis (15) is only rejected in certain of the long-lag models.[18] Further evidence favoring the shorter lag length is again provided by the large oscillations and magnitudes of the coefficients of anticipated and unanticipated components in some of the long-lag models. There is also little tendency for coefficients to die out as the lag distribution is lengthened.

The results of testing the AUDI hypothesis are shown in Table 5. The most striking difference between these results and those in Table 2 are for Barro and Rush's original model. We recall that in the TS model the hypothesis was only rejected once at the 5 percent level, when $m = 15$, and it was

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[18] These are: model BR0 with lag length of 15 and 20; BR2, when $m = 18$, 20; M1, when $m = 18$.

TABLE 5—p VALUES FOR χ^2 TEST OF THE DS MODELS

Model	Expectations Proxy	Number of Lags (m) 7	8	10	12	15	18	20
Output Equation	BR0	.03	.017	.024	.010	[a]	.001	.005
Barro-Rush	BR1	.186	.176	.196	.178	.120	.066	.024
	BR2	.370	.122	.150	.230	.140	.001	[a]
Output Equation	M0	.470	.803	.589	.532	.264	.150	.377
Mishkin	M1	.139	.369	.268	.250	.059	.014	.063
	M2	.390	.705	.426	.426	.129	.150	.241

[a] Indicates a p value less than .005.

TABLE 6—p VALUES FOR χ^2 TESTS $\tau = 0$

Detrending Method		Number of Lags (m) 7	8	10	12	15	18	20
DS	Barro-Rush	.001	.001	.003	.010	.031	.020	.029
	Mishkin	.001	[a]	.001	.002	.009	.005	.009
TS	Barro-Rush	[a]	[a]	[a]	[a]	[a]	[a]	[a]
	Mishkin	[a]	[a]	[a]	.001	.006	.005	.010

[a] Denotes a p value of less than .005.

soundly accepted at shorter lag lengths. In contrast, the AUDI is rejected here for the BR0 model at all lags at the 3 percent level. This change in the results appears to be traceable to the near singularity of the design matrix discussed in Section II. To check this further, we reestimated the DS model using the BR0 specification, but dropped the lagged values of the federal government spending and the military conscription variable, thus permitting the design matrix to change. The result was that for all lag lengths the p value for testing $\theta = \beta$ was in excess of 21 percent. As would be expected, this perturbation of the design matrix led to large changes in the estimated coefficients. It is worth emphasizing that this change in specification did not materially alter the results for any of the other models. The results of the other models tell the same story as those for the TS versions of the output equation.

In the standard specification of the output equation, which has been utilized in the literature on neutrality and rationality, we have now experimented with a variety of specifications of expectations and the natural rate of output. In this framework, the hypothesis that there is no distinction between the output effects of anticipated and unanticipated money growth is unambiguously supported in the models with lag distributions that are three years or shorter. To maintain that the distinction is statistically relevant in this context one has to select particular specifications among the long-lag distributions and expectations mechanisms. However, as we have already argued, the shapes of the lag distributions estimated in those models are difficult to justify on economic and statistical grounds.

IV. Real Output and Money

The empirical results of this paper suggest that the standard specification of the output equation that has been used in the literature in testing neutrality and rationality reduces to equation (5) in which no distinction is made between anticipated and unanticipated money growth. The estimated DS and TS specifications are arrived at by the same substitutions for y_{nt} and u_t used to derive the equation estimated above. Tests of the relevance of lags beyond the eighth (analogous to (15)) yielded results that unambiguously favored the short lag models. The p values were all in excess of 65 percent. Thus, evidence against the use of long lags appears once again. We suggest that it is so much stronger here, because the estimated models use up substantially less degrees of freedom.

Table 6 show that the hypothesis $\tau = 0$ is unambiguously rejected in all models. Thus, in this framework raw money growth appears to have an unambiguous effect on the short-run behavior of output.

V. Concluding Remarks

This paper has reexamined the relationship between output and anticipated and unanticipated money growth in the empirical framework typically used for addressing this question. Our results demonstrate the stylized fact that raw money growth affects real output in the short run, irrespective of whether it is rationally anticipated or not. Thus, while many recent theoretical models have stressed the difference between the effects of anticipated and unanticipated variables, here this difference does not appear to constitute a significant empirical regularity, in pressing need of explanation.

The implications of our results for the rational expectations and policy-neutrality hypotheses have already been discussed in the introduction. The issue arises of which models of the effects of monetary policy on output are compatible with the results of this paper. Apart from the textbook IS-LM model, one such model is the model incorporating adaptive expectations proposed by Milton Friedman (1970, pp. 54–55) in his analysis of the short-run adjustment of real output to changes in nominal variables. As is well known from the work of Sargent and others, if expectations are actually of the fixed weight univariate autoregressive variety, then an observational equivalence problem arises:[19] even if there is a difference between the channels through which anticipated and unanticipated money affect output (i.e., AUDI is not valid), output still depends on distributed lags of money growth and real output. This is the same as the equation that results if expectations are rational and AUDI is valid.

While the standard framework used in studies of neutrality and rationality delivers the AUDI hypothesis, this of course is not the end of the story. More general specifications may reveal that AUDI is invalid when the coefficients of the output equation are allowed to vary over time, or that output depends on variables other than lags of the money growth rate and the hypothesized components of the natural rate. Furthermore, the results in this paper should be integrated with findings based on the VAR methodology. Christopher Sims (1980), and Robert Litterman and Laurence Weiss (1985) find that the effect of innovations in money on output is mitigated when interest rates are included in the system, although David Runkle (1985) shows this outcome to be sensitive to seemingly innocuous changes in specifications. Our results suggest that several other factors merit consideration before a verdict on the role of money can be reached. Candidates appear to be an appropriate method of measuring innovations in money growth in the light of the structural instability evidenced by that process, and the appropriate method of removing trend-related nonstationarity from the data. These are topics for further research.

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[19] This problem also occurs if expectations of money growth depend on lags of real output as well as lags of money growth.

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Inflation, Fixed Cost of Price Adjustment, and Measurement of Relative-Price Variability: Theory and Evidence

By LEIF DANZIGER*

In an interesting paper, Julio Rotemberg (1983) extends the works of Eitan Sheshinski and Yoram Weiss (1977) and Michael Mussa (1981) to examine the aggregate consequences of monetary growth in an economy in which sellers incur a fixed cost of price adjustment. The fixed cost prevents a continual adjustment of individual prices, so each seller keeps his price constant in periods of discrete length, increasing his price in discrete jumps at the end of these periods. A central feature of the inflationary process is therefore that relative prices become dispersed around their means, and the theory predicts that even a fully anticipated inflation increases the variability of relative prices.[1]

The empirical evidence on this point does, however, appear to be mixed. On the one hand, Richard Parks (1978) and Stanley Fischer (1981a,b; 1982) conclude that anticipated inflation does increase relative-price variability (which Fischer interprets as providing empirical support for the theory); on the other hand, Mario Blejer (1981, 1983) and Blejer and Leonardo Leiderman (1982) find that anticipated inflation has no effect, or only a very weak effect on relative-price variability (which makes John Taylor, 1981, doubt the quantitative significance of the theory).[2]

This paper uses Rotemberg's theoretical framework to demonstrate that the proxy for relative-price variability adopted in the empirical literature does not properly capture the relative-price variability that is caused by a fixed cost of price adjustment. The methodology of the above cited studies is therefore not appropriate for evaluating the empirical relevance of the fixed-cost theory, and the divergence of their results is not surprising. Specifically, the proxy which consists of the variance of the rates of price change between successive observations does not always increase with the anticipated inflation. Even with disaggregate data,[3] the proxy decreases with the anticipated inflation in approximately *half* of any interval of inflation rates for which a seller adjusts his price no more than a given number of times between successive observations. Moreover, the relationship between the anticipated inflation and the proxy depends on such extraneous factors as the timing of the observations. A change in the timing may change a positive relationship between the anticipated inflation and the proxy to a negative one, and vice versa. The paper also proposes alternative, more satisfactory measures of relative-price variability and provides empirical support for the theoretical conclusions.

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[1] The connection between inflation and the variability of relative prices in the presence of a fixed cost of price adjustment is also analyzed in Sheshinski and Weiss (1983), Timur Kuran (1983), and myself (1983, 1984, 1987).

[2] Other theoretical and empirical works on the connection between inflation and the variability of relative prices include Herbert Glejser (1965), Daniel Vining and Thomas Elwertowski (1976), Robert Barro (1976), Alex Cukierman (1979), Michael Bordo (1980), and Zvi Hercowitz (1981). A comprehensive discussion of the different theories can be found in Fischer (1981a), and the literature is surveyed in Jamie Marquez and Vining (1984).

[3] It is well understood that aggregation tends to decrease the degree of relative-price variability that can be detected in the data. Although the empirical studies are based on average prices for broadly defined product categories, and not on prices for individual products, this paper intentionally assumes that disaggregate data are used to calculate the value of the proxy. This will prove that the proxy does not perform satisfactorily even under the most favorable circumstances.

I. Rotemberg's Model

Rotemberg considers an economy with many sellers. Each seller produces a unique, nonstorable product and sells it under conditions of monopolistic competition. The sellers face similar demand functions, have the same cost of production, and incur the same fixed cost of adjusting the price. The money stock grows at a constant rate $\Pi > 0$, but the economy is otherwise stationary and there is no uncertainty.

The demand facing seller i, $i \in [0;1]$, at time t is

$$Q_{it} = (P_{it}/P_t)^{-b}(M_t/P_t)^f,$$

where Q_{it} is the quantity demanded from seller i, P_{it} is the price charged by seller i, M_t is the size of the money stock, P_t is the aggregate price level defined by

$$P_t = \exp\left\{\int_0^1 \ln P_{it}\, di\right\},$$

and $b > 1$ and $f > 0$ are constants. Thus, the quantity demanded decreases with the relative price P_{it}/P_t and increases with the real money balances M_t/P_t.

It is assumed that each seller satisfies all demand for his product. The production cost of seller i at time t is

$$\tfrac{1}{2}UP_tQ_{it}^2,$$

where $U > 0$ is a constant. The instantaneous profit of seller i is therefore $P_{it}Q_{it} - \frac{1}{2}UP_tQ_{it}^2$. Differentiating with respect to P_{it} shows that the instantaneous profit is maximized for

$$P_{it}^* = \left(\frac{bU}{b-1}\right)^{1/(1+b)}\left(\frac{M_t}{P_t}\right)^{f/(1+b)} P_t.$$

The actual price usually deviates from the profit-maximizing price due to the fixed cost of price adjustment, and Rotemberg (1982) shows that the instantaneous loss of profit from charging the price P_{it} instead of P_{it}^* is approximately

$$B(\ln P_{it} - \ln P_{it}^*)^2,$$

where $B = k(M_t/P_t)^{2f/(1+b)}$, and k is a positive constant that depends on b and U. Accordingly, the instantaneous loss of profit increases with the real money balances and with the squared difference between the logarithm of the actual price and the logarithm of the price that would maximize the instantaneous profit.

The objective of each seller is to maximize his average profit (net of the cost of price adjustment) within a period with constant price, and he does so by choosing, at each price adjustment, the new price and the length of the period until the next price adjustment. Assuming that sellers are uniformly distributed over the time of their last price adjustment (within the interval $[t-T;T]$), Rotemberg proves that Π is not only the rate of growth in the money stock, it is also the (fully anticipated) rate of inflation. The real money balances are therefore constant, P_{it}^* grows at rate Π, and B is constant.

Due to the stationarity and symmetry assumptions, all sellers choose the same price adjustment policy. Moreover, all periods between price adjustments have the same length, and the new price at the time of a price adjustment is such that the relative price is the same at the beginning of all periods. If $A > 0$ denotes the fixed cost of price adjustment, it follows from Mussa (1981) and Rotemberg (1983) that each seller will keep his price constant in periods of length

$$T = a\Pi^{-2/3}, \tag{1}$$

where $a = (6A/B)^{1/3}$, and charge the price $P_{it} = P_{i\tau}^* e^{\Pi T/2}$ within a period starting at time τ.

II. The Empirical Proxy for Relative-Price Variability

As mentioned, the usual practice in empirical work is to measure the relative-price variability as the variance of the rates of price change between successive observations. That is, the empirical proxy for relative-price variability has the following form

$$V=\int_0^1 w_i(\mu_i-\mu)^2\,di.$$

Here, μ_i is the rate of change in the price of the ith seller's product between successive observations. It is calculated as the difference between the logarithm of the price at successive observations. Therefore, if h denotes the duration of the period between observations, then $\mu_i=\ln P_{i(t+h)}-\ln P_{it}$. Similarly, μ is the rate of change in the aggregate price level between successive observations, and it is calculated as the difference between the logarithm of the aggregate price level at successive observations; that is, $\mu=\ln P_{t+h}-\ln P_t=\ln P_t+\Pi h-\ln P_t=\Pi h$. Finally, w_i is the weight of the ith seller's product, and $\int_0^1 w_i\,di=1$. Since the average expenditure share is the same for all products, $w_i=1$ for all i.

The period between successive price adjustments decreases with the inflation rate (see equation (1)), so there exists a unique inflation rate $\bar{\Pi}$ for which the duration of a period between successive observations is identical to the duration of a period between successive price adjustments. By substituting $h=T$ into equation (1) one obtains that $\bar{\Pi}$ and h are related by $\bar{\Pi}=(a/h)^{3/2}$. It is assumed in this section that $\Pi\le\bar{\Pi}\Leftrightarrow h\le T$, so that each seller increases his price at most once between successive observations. This simplifies the derivations without affecting the essential features of the proxy. The analysis is generalized in the next section where it is shown that the conclusions hold also for $\Pi>\bar{\Pi}\Leftrightarrow h>T$.

Any seller that adjusts his price once from one observation to the next will increase his price by a factor of $e^{\Pi T}$ in order to restore the relative price at the time of the price adjustment to its level at the time of the previous price adjustment. The observed rate of change in the price of such seller is therefore $\mu_i=\Pi T$. The other sellers do not adjust their price between the observations, and the observed rate of change in the price of such seller is $\mu_i=0$. Since the sellers are assumed to be uniformly distributed over the time of their last price adjustment, the proportion of sellers that increase their price from one observation to the next is h/T. The proxy for relative-price variability therefore becomes

$$V=(\Pi T-\Pi h)^2h/T$$
$$+(0-\Pi h)^2(1-h/T)=\Pi^2h(T-h),$$

which by substitution of T from equation (1) and use of the definition of $\bar{\Pi}$ may be written as

$$(2)\quad V=\Pi^2h(a\Pi^{-2/3}-h)$$
$$=a^2(\Pi^{4/3}\bar{\Pi}^{-2/3}-\Pi^2\bar{\Pi}^{-4/3}).$$

To determine the properties of the proxy for relative-price variability as a function of the inflation rate, note that $\lim_{\Pi\to 0}V=0$. In the limiting case of no inflation, there are no changes in individual prices, and a fortiori, no relative-price variability. Note also that if $\Pi=\bar{\Pi}$, then $V=0$. In this case, the duration of the period between successive observations exactly equals the duration of the period between successive price increases. Since all sellers increase their price once from one observation to the next, all observed rates of price increase are exactly equal to the rate of increase in the aggregate price level. Quite naturally, the proxy which is concerned exclusively with the variability of the rates of price change reports the absence of relative-price variability not only when $\Pi=0$, but also when $\Pi=\bar{\Pi}$.

In all other cases, that is, if $\Pi\in(0;\bar{\Pi})$, some sellers increase their price while others keep their price constant between successive observations, so $V>0$. Thus it is clear that the proxy does not always increase with inflation. The same theory which predicts that relative-price variability increases with inflation also predicts that the proxy must decrease with inflation for some Π. More specifically differentiating V with respect to Π shows that V increases in Π for $\Pi\in(0;(2/3)^{3/2}\bar{\Pi})$, obtains a maximum for $\Pi=(2/3)^{3/2}\bar{\Pi}$, and decreases in Π for $\Pi\in((2/3)^{3/2}\bar{\Pi};\bar{\Pi})$. The precise shape of V is shown in Figure 1.

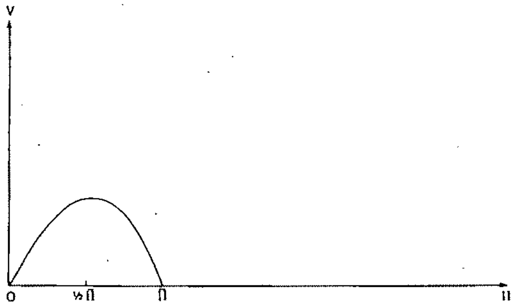

FIGURE 1

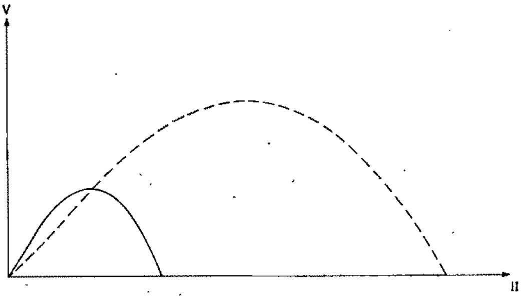

FIGURE 2

The decreasing portion of V covers much more than an insignificant part of $(0; \overline{\Pi}]$. In fact, since $(2/3)^{3/2} \simeq \frac{1}{2}$, it may be concluded that V increases in Π for approximately the first half of the interval $(0; \overline{\Pi}]$ and decreases in Π for approximately the second half of $(0; \overline{\Pi}]$. As a consequence, a fixed cost of price adjustment does not necessarily generate a positive coefficient for the anticipated inflation in a regression equation with the proxy as the dependent variable. Indeed, a zero or negative coefficient is equally consistent with the theory, and regression equations of this type cannot provide much evidence about the empirical importance of the fixed-cost explanation of relative-price variability.

A related point is that the behavior of the proxy depends critically on the timing of the observations. The more frequent the observations, the higher is $\overline{\Pi}$, and the higher is therefore also the inflation rate at which V starts to decrease in Π. To illustrate, Figure 2 depicts V as a function of Π both as it appeared in Figure 1 and as it would appear if the period between successive observations were half as long as in Figure 1. The change in h by a factor of $\frac{1}{2}$ leads to a change in $\overline{\Pi}$ by a factor of $2^{3/2} \simeq 2.8$. The maximum V is therefore obtained at an inflation rate which is approximately 2.8 times higher than before, and one implication is that for any Π for which V decreases in Π with the less frequent observations, V increases in Π with the more frequent observations.[4] Consequently, another defect of the proxy is that the timing of the observations—a matter utterly extraneous to the inflationary process—determines whether the proxy increases or decreases with the rate of inflation.

As Figure 2 indicates, if the observations are sufficiently frequent, then the proxy exhibits the "desired" positive relationship with the rate of inflation. The problem lies of course in determining what is meant by "sufficiently frequent." For instance, would the annual data in Parks, Fischer (1981a), and Blejer-Leiderman, the quarterly data in Fischer (1981a,b; 1982), or the monthly data in Blejer (1981, 1983) contain sufficiently frequent observations? Since $\Pi \lesseqgtr (2/3)^{3/2} \overline{\Pi} \Leftrightarrow T \gtreqless \frac{3}{2}h$, it follows that within the stylized model of this paper the answer would be positive for annual data if the period between successive price adjustments exceeds 18 months, positive for quarterly data if the period between successive price adjustments exceeds $4\frac{1}{2}$ months, and positive for monthly data if the period between successive price adjustments exceeds $1\frac{1}{2}$ months, but negative if not.

III. Generalization to All Inflation Rates

This section generalizes the analysis of Section II by determining the relationship between the proxy and the inflation rate for all values of the inflation rate. Observe that for any h and T, there exists a unique natural number j such that $h/T \leq j < h/T+1 \Leftrightarrow (j-1)T < h \leq jT$. That is, between two observations spaced h apart, each seller increases his price either j or $j-1$ times. (Section II considered the case where $j=1$.) The interval of inflation rates corresponding to a given j can be found using equation (1) and becomes $[(j-1)^{3/2}\bar{\Pi};\ j^{3/2}\bar{\Pi}]$.

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[4] It is easy to show that the curves intersect at the maximum V for the less frequent observations, and that the maximum V for the more frequent observations is twice the maximum V for the less frequent observations.

Any seller that increases his price j times will overall increase his price by a factor of $e^{j\Pi T}$ between successive observations, so the observed rate of increase in the price of such seller is $j\Pi T$. Similarly, the observed rate of increase in the price of a seller that increases his price only $j-1$ times between successive observations is $(j-1)\Pi T$. Since the proportion of sellers that increase their price j times is $[h-(j-1)T]/T = h/T+1-j$, the general expression for the proxy becomes[5]

$$\begin{aligned} V &= (j\Pi T - \Pi h)^2(h/T+1-j) \\ &\quad + [(j-1)\Pi T - \Pi h]^2[1-(h/T+1-j)] \\ &= \Pi^2(jT-h)(h+T-jT). \end{aligned}$$

Substituting for T from equation (1) and using the definition of $\bar{\Pi}$,

$$\begin{aligned} V &= \Pi^2(ja\Pi^{-2/3}-h)[h-(j-1)a\Pi^{-2/3}] \\ &= a^2[(2j-1)\Pi^{4/3}\bar{\Pi}^{-2/3} \\ &\qquad -(j^2-j)\Pi^{2/3}-\Pi^2\bar{\Pi}^{-4/3}], \end{aligned}$$

where $\Pi \in ((j-1)^{3/2}\bar{\Pi};\ j^{3/2}\bar{\Pi}]$. This generalizes formula (2). The proxy vanishes for $\Pi = j^{3/2}\bar{\Pi} \Leftrightarrow h = jT$, $j=1,2,\ldots$; that is, for inflation rates for which the duration of the period between successive observations is a multiple of the duration of the period between successive price adjustments. All sellers then increase their price the same number of times between observations, so the rate of change in any individual price is exactly equal to the rate of change in the aggregate price level. For all other inflation rates some sellers change their price j times, other sellers change their price only $j-1$ times, and the proxy is therefore positive.

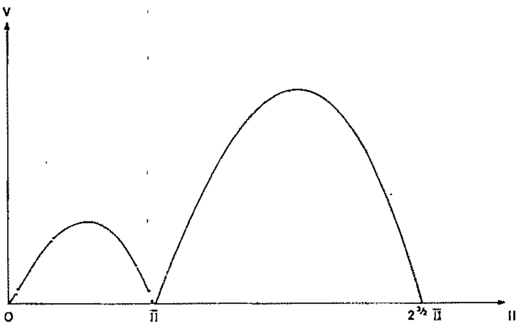

FIGURE 3

Differentiating V with respect to Π shows that V increases in Π for

$$\Pi \in \left((j-1)^{3/2}\bar{\Pi};\ \left\{\left[2j-1+(j^2+1-j)^{1/2}\right]/3\right\}^{3/2}\bar{\Pi}\right),$$

obtains a maximum for

$$\Pi = \left\{\left[2j-1+(j^2+1-j)^{1/2}\right]/3\right\}^{3/2}\bar{\Pi},$$

and decreases in Π for

$$\Pi \in \left(\left\{\left[2j-1+(j^2+1-j)^{1/2}\right]/3\right\}^{3/2};\ j^{3/2}\bar{\Pi}\right].$$

The relationship between V and Π for $\Pi \in (0; 2^{3/2}\bar{\Pi}]$, corresponding to $j \in \{1,2\}$, is shown in Figure 3. The fraction of the interval $(0; j^{3/2}\bar{\Pi}]$ in which V increases with

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[5] It seems worth noting that if the period between observations is prolonged by T, then each seller will increase his price one additional time between the observations. Thus, if h is increased to $h+T$, then j is increased to $j+1$, leaving the value of the proxy unchanged. Accordingly, as a function of h the proxy exhibits a periodicity of T. The reason is that within a period of duration T, both a seller's price and the aggregate price level increase by a factor of $e^{\Pi T}$; the variance of the rates of price change is therefore unaffected by the addition of T to the period between observations.

Π is

$$j^{-3/2}\sum_{l=1}^{j}\left(\left\{\left[2l-1+(l^2+1-l)^{1/2}\right]/3\right\}^{3/2}-(l-1)^{3/2}\right).$$

As shown in Section III, if $j=1$ this fraction equals $(2/3)^{3/2}$ which is close to $\frac{1}{2}$. Further, it may be shown that the fraction decreases in j and approaches the limit value $\frac{1}{2}$ as $j\to\infty$. For any h and corresponding j the interval $(0; j^{3/2}\overline{\Pi}]$ may therefore be divided into two parts of approximately equal size, with the proxy increasing with the inflation rate in one part and decreasing with the inflation rate in the other part. Consequently, the conclusion of the previous section remains unchanged: The empirical importance of the fixed-cost explanation of relative-price variability cannot be determined by examining the sign of the coefficient for the anticipated inflation in a regression equation with the proxy as the dependent variable.

IV. Alternative Measures of Relative-Price Variability

The negative result of the two previous sections does not mean that the relationship between inflation and relative-price variability cannot be uncovered. With the assumptions of the model satisfactory measures of relative-price variability can be calculated from the same data that were used to calculate the proxy. Indeed, even though the period between successive price adjustments is not observed directly, it is possible to determine measures of relative-price variability that are both monotonically increasing in the anticipated inflation and independent of the timing of the observations.

One simple measure is $W=\Pi T$, which represents the amplitude of the changes in relative prices. W is the logarithm of the ratio of the highest relative price to the lowest relative price, either over time for the same seller or over different sellers at the same time. Although W depends on only the extreme values of the relative prices, it captures important aspects of the relative-price variability. In particular, since the policy of price adjustment adopted by the sellers implies that $W=a\Pi^{1/3}$ (see equation (1)), W is both monotonically increasing in the anticipated inflation and independent of the timing of the observations.[6]

When first W has been obtained one can determine other measures of relative-price variability that depend on the whole shape of the distribution of relative prices and are therefore more complex. An example of such measures is the coefficient of variation of the relative prices, again either over time for the same seller or over different sellers at the same time. It may be shown that for the demand and cost functions considered in this paper the coefficient of variation depends on Π and T only through W, in which it increases monotonically (see my 1987 article). Hence with the assumptions of the model the coefficient of variation is also obtainable from the data and a satisfactory measure of relative-price variability.

V. Empirical Results

The empirical properties of both W and V are now briefly examined using monthly samples of retail prices in Israel collected by the Central Bureau of Statistics for the purpose of calculating the Consumer Price Index.[7] The product category chosen for the analysis is the narrowly defined "100 grams of kosher choice salami," and the price data

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[6] The relationship between V and W is easily determined. Let g denote the fraction of sellers that increase their price j times between successive observations. Therefore $g=h/T+1-j \Leftrightarrow jT=h+T(1-g)$. Substituting for jT in the expression for V yields

$$\begin{aligned}V&=\Pi^2[h+T(1-g)-h][h+T-h-T(1-g)]\\&=W^2g(1-g),\end{aligned}$$

which shows that $V^{1/2}$ equals W multiplied by the geometric mean of the fraction of sellers that change their price j times and the fraction of sellers that change their price $j-1$ times.

[7] The sampling and computation techniques are described in the Central Bureau of Statistics (1968).

TABLE 1—AVERAGE MONTHLY INFLATION RATES

Year	Average Monthly Inflation Rates (Percent)
1968	.2
1971	1.0
1972	1.0
1974	3.7
1975	1.8
1976	2.7
1977	3.0
1978	3.3
1979	6.2
1980	7.0
1981	5.8
1982	7.0
1983	8.9
1984	14.1

Source: Calculated from the Central Bureau of Statistics (1985).

were copied from the original files of the Bureau. The selection of this particular product category was guided by the availability of adequate monthly price series for many years, the monopolistic competition in the market for the product category, and the absence of direct government interference with the price mechanism. The years available were 1968, 1971–72, and 1974–84, with the file for each year containing the prices of the sampled sellers in each of the 12 months of that year as well as in December of the previous year.

Before calculating W and V, it is useful to provide some information about the movement in the Consumer Price Index over time. Let $m=1$ denote January, $m=2$ February, etc., and $P(m,y)$ the value of the CPI in month m of year y. By definition, month 0 of year y is December of year $y-1$. The average monthly inflation in year y (i.e., from December of year $y-1$ to December of year y) is then $[\ln P(12,y)-\ln P(0,y)]/12$, and these rates are reported in Table 1 for each year in which the retail prices of salami were available. As can be seen, the inflation rates cover a wide spectrum. In 1968, the aggregate price level was almost stable, while in 1971–72, the inflation rate was a relatively moderate 1 percent per month, increasing to about 2–4 percent per month in 1974–78. This was followed by the still higher inflation rates of about 6–7 percent in 1979–82, and finally by the relatively extreme 9–14 percent in 1983–84.

The CPI has not always increased. Especially in the first years of the sample, the CPI has often fallen from one month to the next. To avoid having to deal with negative inflation rates, the inflation rates are averaged over 2 months.[8] In addition, it is assumed that the anticipated inflation can be represented by the realized inflation. Accordingly, for m even $\Pi(m,y)=\frac{1}{2}[\ln P(m,y)-\ln P(m-2,y)]$ is the anticipated monthly inflation rate in the $(m/2)$th of the six successive 2-month periods in year y.[9] For consistency, W and V must refer to the same 2-month periods.

In order to calculate W, note that since the prices are sampled once a month, the size of the price changes can only be determined if it is assumed that the prices are not adjusted more than once a month. That is, if seller i charges $P_i(m,y)$ for 100 grams of salami the particular day in month m of year y in which he is visited by the price collector, and the same seller charged $P_i(m-1,y)$ for 100 grams of salami when he was visited in the previous month, then it is assumed that the price has increased by $P_i(m,y)-P_i(m-1,y)$ in a single jump. Because the validity of this assumption is questionable with very high inflation rates, the last 4 months of 1983 and all of 1984 have been excluded.[10] The analysis is therefore based on the $12\frac{2}{3}$ remaining years (1968, 1971–72, 1974–82, and the first 8 months of 1983).[11]

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[8] However, there remain three 2-month periods with negative inflation rates. These periods are omitted in the estimations below.

[9] This is not essential. Dividing the realized inflation into anticipated and unanticipated inflation (with the anticipated inflation being generated from the realized inflation using an auregressive model, and the unanticipated inflation being the difference between the actual and the anticipated inflation) leads to virtually the same conclusions. Also, Sheshinski-Weiss (1983) and I (1983, 1984) show that for a given stochastic process of inflation the price adjustments are independent of whether the realized inflation is anticipated or unanticipated. The analysis abstracts from other reasons for price changes than aggregate inflation, and it assumes that price changes from month $m-2$ to month m of year y approximate the price changes in a steady state with inflation rate $\Pi(m,y)$.

The sample of sellers with reported prices may vary from month to month, both because of decisions by the Bureau to change the sample, and because of sellers being temporarily closed or out of stock. Only those sellers with reported prices in two successive months are considered, and W is calculated as the average, over all monthly price changes within each 2-month period, of the difference between the logarithm of the price after a price change and the logarithm of the price before a price change. Let $I(m, y)$ be the subsample of sellers that charged different prices at the price collector's visit in months m and $m-1$ of year y. Accordingly, for m even the value of W in the $(m/2)$th 2-month period of year y is calculated as[12]

$$W=\left\{\sum_{i\in I(m,y)}[\ln P_i(m,y)-\ln P_i(m-1,y)] + \sum_{i\in I(m-1,y)}[\ln P_i(m-1,y) - \ln P_i(m-2,y)]\right\}/ \left[|I(m,y)|+|I(m-1,y)|\right].$$

The model predicts that the amplitude of the relative-price changes increases with anticipated inflation, and for the specific functional forms, that W is a linear function of $\Pi^{1/3}$. Regressing W on $\Pi^{1/3}$ yields the following estimates:

$$\hat{W} = -.03 + .35\Pi^{1/3} \quad D.W. = 1.93,$$
$$(-1.8)\ (7.9) \qquad R^2 = .47$$

(t-statistics are shown in parentheses). The coefficient of $\Pi^{1/3}$ is positive and statistically very significant. Accordingly, it may be concluded that relative-price variability as measured by W increases monotonically with the anticipated inflation.[13]

To calculate V assume that m is even and let $J(m, y)$ be the subsample of sellers with reported prices in both month m and month $m-2$ of year y. The value of V in the $(m/2)$th 2-month period of year y is[14]

$$V = \sum_{i\in J(m,y)}[\ln P_i(m,y) - \ln P_i(m-2,y) - 2\Pi(m,y)]^2/|J(m,y)|.$$

The ratio of all sellers with reported prices in two successive months to the number of sellers which change their price between the same 2 months varies from 18.0 in 1968 to 1.6 in the first 8 months of 1983, and is on average 3.0. Since this ratio is less than the number of months in which the average seller keeps his price constant, it seems reasonable to assume that for the average seller the period with constant price almost always exceeds 2 months. Consequently, for the relevant values of inflation, the model predicts that V first increases and then decreases with Π, and for the specific functional forms, that V is a linear function of $\Pi^{4/3}$ and Π^2. Regressing V on $\Pi^{4/3}$ and Π^2 yields the following estimates

$$\hat{V} = .001 + 1.18\Pi^{4/3} - 4.63\Pi^2$$
$$(.8) \quad (4.4) \qquad (-3.3)$$
$$D.W. = 2.06, \quad R^2 = .45,$$

(t-statistics are shown in parentheses). The coefficients of both $\Pi^{4/3}$ and Π^2 have correct signs and are statistically very significant. The estimated value of V increases in Π for $\Pi < .070$ and decreases in Π for $\Pi > .070$. There are twelve 2-month periods (out of 73) on the decreasing part of V, and the estimates imply that V becomes zero (prices adjusted every two months) for $\Pi = .129$. Since the average monthly inflation rate in any of the 2-month periods is less than .094, this is consistent with the assumption that the average seller keeps his price constant more than 2 months, and it may be concluded that V does not increase monotonically with the inflation rate. The empirical findings therefore support the theoretical predictions of the model.

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[10] The inflation takes off toward the end of 1983, with the average monthly inflation rate being less than 9.4 percent in any 2-month period prior to September 1983, but exceeding 12.5 percent in any later 2-month period.

[11] This yields $12\cdot 6+4-3=73$ 2-month periods with positive inflation rates. On average, 40.8 prices were collected each month.

[12] $|I(m,y)|+|I(m-1,y)|$, the number of sellers in $I(m,y)$ and $I(m-1,y)$, is always positive and on average equal to 25.0.

[13] The intercept is negative and statistically significant at the 7.4 percent level. This result is, however, very sensitive to the specific functional form. For example, if W is regressed on $\Pi^{1/2}$, then the intercept is statistically insignificant while the coefficient of $\Pi^{1/2}$ is positive and statistically very significant.

[14] $|J(m,y)|$, the number of sellers in $J(m,y)$, is on average equal to 37.0.

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Black-White Differences in Schooling Investment and Human Capital Production in Segregated Schools

By PETER F. ORAZEM*

The lower level of school quality available for blacks relative to whites in the segregated era is frequently cited as a primary cause for the currently observed gap in black-white average wages. The inferior education provided to black children is argued to have caused lower levels of human capital production in black schools than white schools. The gap in black-white wages can be traced to this gap in human capital. Similarly, the convergence in black-and-white average wages during the 1960's and 1970's may be explained by the steady convergence in black-and-white school quality and attendance that began in the 1940's.[1]

Given the presumed importance of school quality differences in explaining both the existence of and the changes in black-white wage differentials, little is known about the human capital production process in the segregated schools. While studies of educational production processes exist, virtually all cover periods following the 1954 Supreme Court ruling against "separate-but-equal" schools. We therefore have no direct evidence regarding the extent to which differences in black-white school characteristics may explain the differences in human capital investment and production between black-and-white children in the segregated era.[2]

The objective of this paper is to present an econometric analysis of the determinants of achievement (measured by test scores) and attendance in black-and-white segregated schools, utilizing data from Maryland grade schools from 1924 through 1938. The results show that black-white differences in school characteristics explain virtually all of the differences in school attendance between black-and-white children in the period. Second, the determinants of achievement in black-and-white schools were not statistically different. This implies that, had school quality been equalized between blacks and whites, their school achievement would have been more equal, and the remaining differences in student achievement would have been due to other factors such as differences in socioeconomic backgrounds. Differences

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[1] Finis Welch (1973) and James Smith and Welch (1977) are prominent proponents of this "vintage" explanation for the convergence in black-white average wages over the past two decades. This view has been challenged on two fronts. Richard Freeman (1973) argues that increasing relative demand for black workers due to governmental action to reduce discrimination has caused the gap in black-white wages to narrow. Edward Lazear (1976) contends that the narrowing gap is merely an exchange of higher entry-level wages for blacks for lower wage growth later in life.

[2] If the production process were stable, we could use the results from more recent studies to make inferences about school achievement in the segregated era. Unfortunately, there is little agreement as to which characteristics are important in the human capital production process. Even if recent studies agreed on some set of school characteristics, these studies may give a misleading picture of human capital production in earlier periods due to the convergence in school quality measures across school districts over time. As an example, the characteristics Welch (1973) uses to illustrate the convergence of school quality across racial groups, namely school attendance, teacher salaries, length of school term, and expenditures per pupil, generally have small measured effects in studies of the determinants of average child achievement across school districts. It would be wrong to conclude from such studies that these characteristics do not affect student achievement. The small or statistically insignificant parameters for these characteristics do not imply that they are unimportant in determining the *level* of achievement; only that they are unimportant in explaining the *differences* in achievement across school districts.

in school quality accounted for an average of 37 percent of the observed differences in student achievement in the sample period. Finally, the school characteristics that proved to be important in explaining attendance and achievement in this sample exhibited tendencies to converge across races later in the century, signaling a convergence in human capital production between blacks and whites. These findings provide strong support for the role of school quality in helping to explain the differences in human capital investment and production and the resulting differences in returns to schooling between blacks and whites.

The discussion will proceed as follows: Section I presents a modeling strategy for analyzing school attendance and human capital production. Section II discusses the data. The estimation is reported in Section III.

I. Theory

We assume that parents make the decisions regarding the proportion of time their child will spend in school. This decision is based on a utility maximization framework in which parents derive satisfaction from their child's achievement in school. Parents are assumed to know the learning process by which school characteristics and child time spent in school are translated into human capital. By imbedding this educational production process into the parents' maximization problem, we can derive a demand function for schooling which shows that a child's attendance is determined by school characteristics, the child's wage, the parents' wage, and family assets. This attendance equation has the general form

$$(1)\qquad A = f(w^p, w^c, Y, S),$$

where A is child attendance, measured on a continuum between 0 and 1 with 1 being full-time attendance, Y is the family's nonwage assets, w^p is the parents' wage, w^c is the child's wage, and S is a vector of school characteristics which are exogenous to the parents. The opportunity cost of a child's attendance is the value of the child's labor either at home or as an employed worker outside the home.[3] As the value of child labor rises, the incentives to reduce child attendance would increase if income effects of the wage increase are larger than the substitution effects. If child achievement in school is a normal good, increases in parental wages or assets should increase school attendance. The effect of school quality on school attendance is ambiguous.

The production process for human capital produced in the school year, Q, is assumed to depend upon prior accumulations of human capital K_0, a vector of home inputs into the production process H, and the vector of school inputs S. The H and K are assumed to be fixed before the school year begins. The H represents inputs such as the socioeconomic status of the family, parental educational attainment, and the influence of the child's peers or siblings. The child has access to these inputs regardless of whether or not he attends school. However, school inputs are available to the child only if he attends school. This implies that the child's proportion of time spent in school is the proportion of the school characteristics actually used in human capital production. We can express the production of human capital within the period as

$$(2)\qquad Q = g(AS, H, K_0).$$

Multiplying the vector of school characteristics by attendance is done to capture the intensity of investment in schooling. This specification is analogous to the use of utilization rates for capital and labor in studies of industrial production. In the application here, if A equals one, meaning that the child attends full-time, (2) reduces to the "value added" production specification commonly used in the educational production literature. Because daily attendance varied substantially in Maryland in the 1920's and

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[3] Here, the child wage in household work is assumed to be equal to the child wage in market work. Since the empirical work is based on a largely rural population, most child labor would be household or farm chores, either in their parents' or their neighbor's home.

1930's, both across time, across county school systems, and across races, the assumption of equal intensity of schooling investment cannot be maintained.

II. Data and Empirical Strategies

First-order approximations of equations (1) and (2) are applied to Maryland data consisting of county averages of school attendance and achievement over the period 1924 through 1938. The data were culled from the *Annual Reports of the Maryland State Board of Education*.[4] The Maryland data are uniquely suited to this study because the State Board of Education kept detailed and consistent statistics on school characteristics for both the 22 black and 23 white county school systems throughout the segregated era. There were no black schools in Garret County. The Baltimore city school system was not under the state system, and is excluded from the sample. The period 1924 through 1938 is used because reading test scores for both the black-and-white schools were available for certain years in that period. Although testing continued in later years, the results of those tests were not preserved.

The empirical analysis is restricted to the primary school data. The main reason is that grade schools represented most if not all of the schooling that children obtained.[5] A second reason is that, because grade schools concentrate more on a core curriculum, a single measure of school output may reasonably characterize the human capital produced in the school. At the secondary level, the curricula become more diverse, and thus it becomes more difficult to adequately portray the school's learning output with one measure of achievement.

Definitions and summary statistics for the variables are presented in Table 1. The dependent variable in the demand equation, *ADA*, is the ratio of average daily attendance to the number of children enrolled in the county grade school system. Black student attendance averaged 7 percentage points below that of white students in the period. The difference in average daily attendance narrowed steadily over the period from 9 percentage points in 1924 to 3 percentage points in 1938, even though white attendance was increasing rapidly.

The dependent variable in the production function is measured as the proportion of students taking a nationally standardized test of reading skills who meet or exceed the national norm for the test. Although the tests differ from year to year, referencing over a common standard of national achievement makes comparison of tested ability feasible. The tests were generally given over the grades 2 through 7, so achievement represents the average achievement in the grade schools as a whole. Tests were given in the white schools in years 1924, 1928, 1932, and 1935. They were administered in the black schools in years 1924, 1932, 1935, and 1938. In total, there are 92 observations for the white schools and 82 for the black schools.[6] Over the period, white test results clearly exceeded black test results, although black students progressed at a more rapid rate. In the two years in which identical tests are available, the ratio of black-to-white student achievement rose from .26 to 1924 to .45 in 1935, even though the proportion of white students meeting national norms had risen from 44 percent in 1924 to 59 percent in 1935. In other words, relative black performance on these exams increased despite improvements in white student performance over the period.

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[4] For a detailed description of this data, see my dissertation (1983).

[5] The average attained level of schooling for the cohort entering the labor force in 1930 was 9.6 years for whites and 5.8 years for blacks. The proportion of the population with less than 9 years of schooling was 42 percent for whites and 78 percent for blacks. These figures are for the entire United States. Southern states generally lagged behind the national average in schooling, so the average years of attained schooling for Maryland are probably somewhat lower.

[6] There were two missing observations for black schools in 1924 and 1932. Two black school observations were excluded in 1938 as outliers.

TABLE 1—EMPIRICAL DEFINITIONS AND SUMMARY STATISTICS FOR VARIABLE USED IN ESTIMATING THE SCHOOLING DEMAND AND PRODUCTION EQUATIONS

	Definition	Black Mean	White Mean
Endogenous Variables			
ADA (D)	Average daily attendance in public grade school as a proportion of enrollment	.76	.83
TEST (P)	Proportion of students taking a standardized test of reading skills who meet or exceed the national norm	.225	.54
Exogenous Variables			
EXP1 (D&P)	Proportion of teachers with at least one year of teaching experience	.95	.90
TERM (D&P)	Length of school term	168.9	185.4
CERT (D&P)	Proportion of teachers with at least a normal school certification	.86	.90
SMLSCH (D&P)	Proportion of schools with only one or two teachers	.90	.72
VALUE (D)	Value of school buildings and equipment per enrolled student	.056	.161
(P)	Value of school buildings and equipment per attending student	.07	.193
PTRAT (D)	Number of enrolled students per teacher	37.0	35.5
(P)	Number of attending students per teacher	27.9	29.4
WAGE (D)	Proxy variable for child wage. The measure used is the value of crop production per acre per county per year (1919 dollars)	.24	.24
ASSET (D)	Average value of farm land and buildings per farm per county per year (1919 dollars)	23.56	51.28
SALARY (D)	Index of annual salaries in manufacturing (1919 dollars)	.72	.72
DUM31 (D)	Dummy variable equal to 1 for years preceding 1931		
RACE (P)	Dummy variable equal to 1 for black schools		

Note: (D) signifies a demand variable and (P) signifies a production variable.

The school characteristics include measures of pupils per teacher, school property value, length of term, teacher education and experience, and school consolidation. In the demand function, *PTRAT* is the proportion of students enrolled per teacher. For the production function, *PTRAT* is the proportion of students in average attendance per teacher. The difference arises from the use of a different sample of students in the demand equation than in the production equation. The full sample of enrolled pupils is used in the demand equation, but the intensity of input use in the production equation depends upon the students actually attending class. Similarly, *VALUE* is the assessed valuation of school property and equipment per enrolled student for the attendance equation and per average attendance for the achievement equation. *TERM* is the length of the school term, *CERT* is the proportion of teachers with at least a normal school (two-year teachers college) education, *EXP1* is the proportion of teachers with at least one year of experience,[7] and *SMLSCH* is the proportion of schools with only one or two teachers. The last four variables are measured identically in the two equations.

Child wages are measured by the proxy variable, *WAGE*. The variable *WAGE* captures variations in the value of labor on farms due to yearly fluctuations in agricultural prices or agricultural productivity.[8] In years when agricultural prices are high or crop yields are large, the child wage (the child's value as a laborer in the home) rises as does the opportunity cost of schooling. Adult wages are measured by the variable, *SALARY*, which is an index of yearly manufacturing salaries in Maryland. Child labor laws prevented children from taking manufacturing jobs, so this variable should capture movements in adult wages which are independent of movements of child wages. *ASSET* is a measure of the average value of land and buildings per farm per county per year. These figures were reported separately for nonwhites and whites in Maryland in the *U.S. Census of Agriculture* every five years. For the intervening years, assets were interpolated. *SALARY* and *ASSET* would have pure income effects on attendance, so they should both increase attendance if education is a normal good.[9]

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[7] Average experience levels for the teachers were not available at the county level for the entire sample period, so the proportion of teachers with some experience was used. This measure has also been utilized by Eric Hanushek (1972) and Richard Murnane (1975).

The other variables control for cost variation in schooling across counties and time. A county dummy variable for each was included to capture county-specific, time-invariant costs of schooling. These are attributed to differences in local transportation costs, labor markets, population density, crop mix, and community literacy rates. An additional variable, *DUM31*, is a dummy variable controlling for potential differences in schooling demand preceding a change in the truancy laws in 1931. Its coefficient should be negative because the earlier, less stringent laws would correspond to higher opportunity costs of child time in school.

For the production function, the vectors of home inputs common to more recent studies are not available for this study. This lack of data may not be a serious problem here. Commonly used measures of home inputs into child achievement do not vary markedly over time. Thus, measures such as parents' education or occupation are assumed to be fixed over time and captured by county-specific dummy variables. Because of the degree of racial segregation in communities, jobs, churches, and schools during this period, *RACE*, a dummy variable conditioning on the racial designation of the segregated school system, should more adequately control for differences in social or peer influences than the same variable would for current school systems.

Past achievement is also unavailable because tests were not given in concurrent years. However, our measure of achievement, the proportion of the population exceeding the national norm for that grade at the conclusion of the year, helps to mitigate somewhat the potential missing variables problem. Standards for passing from one grade to the next divide the population into categories of previously accumulated human capital, namely those who have attained the level necessary to enter the second grade, the third grade, and so on. This is admittedly a weak control for lagged achievement, so the problem of omitted variables bias may be only partially corrected by the measure of achievement used.

III. Results

This section reports the results obtained from the estimation of the educational demand and educational production equations. The results are used to identify school characteristics that were important in explaining differences in black-and-white attendance and achievement in the period. Finally, the timing of the convergence of these black-and-white school characteristics is discussed.

A. *School Attendance*

The attendance equations are reported in Table 2. The regressions are reported sep-

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[8] The use of a measure of the value of child labor in agriculture stems from the rural nature of Maryland population at the time (the United States Census designates 67 percent of the population as rural) and because of the exclusion of Baltimore City from the sample. The correlation between the proxy measure of wages at the state level and an index of agricultural wages was .94.

[9] The use of wage and asset data that aggregate over the entire county population rather than only the population with school children introduces measurement error in the attendance equation. Thus, while the parameter estimates of the attendance equation reported below appear quite reasonable, they should be treated with some caution.

TABLE 2—ESTIMATES OF GRADE SCHOOL ATTENDANCE EQUATIONS FOR BLACK-AND-WHITE CHILDREN IN MARYLAND, 1924–1938

Variable[a]	White	Black
EXPi	.0484	.0454
	(4.14)	(.918)
TERM/1000	−.4103	.459
	(1.15)	(.887)
VALUE	.023	.0254
	(1.10)	(.227)
PTRAT/100	.1006	−.2135
	(1.95)	(3.06)
CERT	.1253	.0659
	(13.7)	(4.58)
SMLSCH	−.0506	−.340
	(4.38)	(4.96)
WAGE	−.0476	−.1168
	(2.08)	(3.17)
ASSET/100	.0508	.0315
	(5.12)	(2.33)
DUM31	−.0077	−.0409
	(2.76)	(7.73)
SALARY	.0114	.0785
	(.629)	(2.19)
Mean Fixed Effect	.7210[b]	.9495[b]
R^2	.85	.85
N	345	330
Mean of Dependent Variable	.826[c]	.76[c]

[a] *t*-statistics are in parentheses. The dependent variable is the proportion of enrolled students in average daily attendance.

[b] Fixed effects significant at .01 level of confidence.

[c] Using the black mean school characteristics in the white demand function increases predicted white attendance to .836. Using white mean school characteristics in the black demand function increases predicted black attendance to .838.

arately for blacks and whites since the null hypothesis of equal coefficients across the two regressions was rejected at the .01 level of confidence. The model seems to fit the data quite well for both groups, with 85 percent of the variation in average daily attendance explained by the regressors.

The measures of the child's opportunity cost of schooling and the family's income perform quite well in the attendance regressions. In both systems, increasing family assets significantly increases school attendance. In addition, increasing the average manufacturing salary increases school attendance in the black schools. On the other hand, increasing the value of agricultural labor significantly reduces the proportion of the year that black-and-white children attend school. Black attendance is more than twice as sensitive to fluctuations in the local agricultural economy than white attendance. Finally, the relatively less restrictive truancy laws before 1931 are associated with reduced attendance for both black-and-white children.[10]

Although there are no explicit sign predictions on the school quality measures, it is clear that school quality has an effect on schooling demand. Teacher quality in the form of teacher education and experience has a positive effect on attendance in both school systems. Schools organized with one or two teachers have significantly lower attendance for both racial groups. Higher pupil-teacher ratios significantly lower attendance in the black schools but increase attendance in white schools. School value increases attendance in both systems, while length of term raises black attendance but lowers white attendance. Neither of the last two variables has a coefficient that exceeds its standard error in either school system.[11]

The results in Table 2 demonstrate clearly that attendance was sensitive to economic conditions and school quality measures, even though truancy laws were nominally in place throughout the period. As late as 1938, white students were absent an average of 33 days per year and black students were absent an average of 42 days per year. The average absentee rate among white students was so great that black students who attended full-time would have spent more days in school per year (172 days) than the average white student (160 days) despite the large differences in length of term between the black-and-white schools. In other words, length of term, in and of itself, did not explain the difference in time spent in school between blacks and whites. Black students attended less than white students because their families were poor, because more young blacks had to work, and because their schools were of lower quality.[12]

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[10] Although the coefficients on the wage proxy, manufacturing salary, and average income had the same signs across the black-and-white attendance equations, the null hypothesis that these responses were equal across two equations was rejected at the .01 level of confidence.

[11] The null hypothesis that black-and-white attendance responsed equally to changes in school quality measures was rejected at the .01 level of confidence.

To illustrate how much of an effect school characteristics had on the attendance choices of black-and-white children, we can impose white mean school characteristics on the black attendance equation. This empirical equalization of schools results in an increase in mean daily black attendance from 76 percent of enrollment to 84 percent of enrollment. This compares to daily attendance in white schools over the period of 83 percent of enrollment. Raising black school quality to the level found in white schools appears to eliminate the differences in school investment decisions of blacks and whites, even when the controls for home inputs, wages, and family incomes are unchanged.

B. *School Achievement*

The production parameters estimated over the pooled sample of black-and-white students are presented in Table 3.[13] The first column presents the results without imposing the county-specific fixed effects and constraining the black-and-white production parameters to be equal. The second column adds the county-specific fixed effects. The third column adds the fixed effects and allows the production parameters to differ by race. The county-specific fixed effects are used to help control for unmeasured home or school inputs into the human capital production process. The various specifications can explain about 80 percent of the variation in the proportions of students meeting the national norms on tests of reading skills.

The results in column 1 indicate that teacher certification levels, the per pupil value of school property, and the length of school term are the most important school inputs, both in terms of significance levels and in terms of the magnitude of coefficients evaluated at sample means. When the county-specific fixed effects are added, the size and significance of the teacher certification level drops, but length of term and school property value remain important. For both specifications, the dummy variable controlling for the racial designation of the school is negative and significant. The implication is that the levels of unmeasured school and family inputs into the human capital production process are lower for blacks than whites.

In the third column, the production parameters are allowed to differ between the two segregated school systems. For both systems, the hypothesis that school inputs and attendance do not matter in explaining student achievement was rejected at the .01 level of significance.[14] School property value and teacher certification are the most important school inputs for white schools, whereas length of term and school property value are the most important inputs in the black schools. In addition, the measure of teacher experience has a large impact in the black schools when evaluated at the sample mean, but the coefficient is not measured precisely.

The focus of this study is to determine whether the production parameters differ be-

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[12] Differences in health might also cause differences in attendance between black-and-white children. However, reported health status is likely to be inaccurate since there is an incentive for parents to declare truant children to be ill. Even so, reported health status is quite similar for black and white children. The Maryland *Annual Reports* listed the proportion of students who withdrew for "mental and physical incapacity" starting in 1929. From 1929 through 1938, .8 percent of the black children and 1 percent of the white children withdrew per year for these health reasons. Over the same period, employment and economic reasons caused 2.7 percent of the black children and 1.3 percent of the white children to withdraw per year. These figures suggest that differences in health status account for little of the difference in attendance between blacks and whites, whereas economic factors are responsible for a large proportion of the difference.

[13] The sample was tested for heteroskedastic errors across races and across time periods. The null hypothesis of homoskedastic errors could not be rejected at the .05 level in either case.

[14] For the white schools, the F-statistic was 5.93. For the black schools, the F-statistic was 5.87. The critical value at the .01 significance level is $F(7,137) = 2.78$.

TABLE 3—ESTIMATES OF THE EDUCATIONAL PRODUCTION EQUATIONS FOR BLACK AND WHITE CHILDREN IN MARYLAND, VARIOUS YEARS BETWEEN 1924–1938

	1	2	3	
Variable[a]	Joint	Joint	White	Black
EXP*ADA[b]	.085	−.063	−.107	.463
	(1.05)	(.34)	(.47)	(.71)
TERM*ADA/100	.254	.475	.30	.461
	(1.74)	(2.25)	(.59)	(1.83)
VALUE*ADA	.432	.654	.581	.822
	(3.71)	(2.69)	(2.09)	(1.57)
PTRAT*ADA/100	.164	−.170	.076	.195
	(.49)	(−.41)	(.09)	(.33)
CERT*ADA	.255	.093	.325	−.032
	(3.55)	(.90)	(1.50)	(.23)
SMLSCH*ADA	−.102	−.057	−.125	.401
	(1.24)	(.42)	(.68)	(1.24)
ADA	−.801	−.058	−.811	−.761
	(2.01)	.10	(.59)	(.94)
RACE	−.25	−.166		−1.06
	(7.89)	(3.61)		(1.46)
CONSTANT	.518	−.20		.544
	(3.01)	(.67)		(.63)
County Fixed Effects	—	included		included
R^2	.79	.80		.81
N	174		174	174
$\hat{Q}_{B/\overline{W}}$	.30	.40		.33
Percent Reduction in Gap	24	57		33
$\hat{Q}_{W/\overline{B}}$	.44	.42		.42
Percent Reduction in Gap	30	38		38

[a] *t*-statistics are in parentheses. The years include 1924, 1928, and 1935 for white children, and 1924, 1932, 1935, and 1938 for black children. The dependent variable is the proportion of students taking a standardized test of reading skill who meet or exceed the national norm for the test.

[b] The predicted value of average daily attendance is used in place of actual value of *ADA* for all school inputs to avoid potential biases caused by correlation between attendance and missing school or home inputs into the human capital production process. The estimated parameters using actual attendance were virtually the same in sign, magnitude, and significance.

tween the black-and-white schools. The null hypothesis that the production coefficients across the black-and-white schools are equal cannot be rejected at the .1 level of significance.[15] Furthermore, the conclusion that the black-and-white human capital production processes are equal appears to be robust to changes in the specification of the production function. For example, inserting the elements of the attendance equation (1) into the production equation (2) yields a quite general quadratic form in school characteristics, economic variables, and fixed effects. The complete system would involve 491 parameters and cannot be estimated. However, a partial specification suppressing the interactions with the county-specific fixed effects involved 152 parameters (22 fixed effects, 128 interaction terms, a constant, and a dummy variable for race). The test of equality of the coefficients across races in this partially unconstrained system also could not be rejected at the .1 level of significance.[16]

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[15] The *F*-statistic on the null hypothesis that the black-and-white coefficients on school characteristics and attendance are equal across races was 0.76, which has a marginal significance level of .62.

To determine the importance of school quality in explaining the differences in human capital between black-and-white children, the results in Table 3 were used to illustrate the impact of artificially equalizing school quality in the black-and-white schools. $\hat{Q}_{B/\overline{W}}$ represents the predicted proportion of black students who would have attained the national norm had black school quality been equal to the mean of the white school characteristics. $\hat{Q}_{W/\overline{B}}$ represents the predicted white student achievement at the mean of black school quality. These predicted values and their corresponding predicted reductions in the gap in black-and-white student achievement are reported at the bottom of Table 3. The results are quite consistent across the various specifications. Improving black schools to the level of average white school quality would have raised average black pass rates from 8 to 18 percentage points. Interestingly, the larger predicted pass rates were found in the specifications controlling for the county fixed effects. Between 24 and 57 percent of the gap in black-and-white achievement would have been eliminated by equalizing school characteristics at the levels found in white schools. Similar conclusions are found with an equalization at black mean school characteristics. White pass rates would have fallen 10 to 12 points with the larger reduction in the fixed effect regressions. Between 30 and 38 percent of the gap in achievement would have been eliminated.[17]

It is interesting to consider the avenue by which achievement is affected. In the black schools, an average of 7 percentage points of the improvement can be attributed directly to the improvement in school quality and another 5 percent to the induced increase in black attendance from the improved school quality. Because white attendance was not changed by reducing white school quality to the level found in the black schools, virtually all of the predicted reduction in white achievement was directly attributable to reductions in school quality.

It is important to emphasize that the finding that the school production parameters were equal across the black-and-white schools only implies that human capital production attributable to school characteristics would have been equalized. Differences in the levels or productivities of household inputs would prevent equality in the overall level of human capital production across races.

C. *The Timing of the Convergence in Black-and-White School Characteristics and Achievement*

By 1935, the last year for which direct comparisons between black-and-white schools can be made, there were two black school systems with over half of their students meeting or exceeding national norms. Four black systems, Allegany (57.8 percent), Wicomico (56.2 percent), Somerset (49.4 percent), and Baltimore county (48.1 percent), had achievement rates that exceeded rates in the worst white school systems. Still, in no county system did black achievement reach the level of white achievement. The best black schools tended to be in counties with the best white schools, and the white schools were consistently of higher quality. Although considerable reductions in the disparity between black-and-white school quality occurred between 1924 and 1938, and although average black achievement relative to white achievement rose from .26 to .45 over the 1924–1935 period, substantial differences between the school systems remained.

The attendance and production function estimates indicated that teacher certification, teacher experience, length of term, proportion of one and two teacher schools, and value of school property and equipment were important in the human capital production process in the segregated era. By 1938, teacher experience and certification levels had already become equal in Maryland. Length of term was equalized in Maryland grade schools by 1943. Although black schools gained relative to white schools, in the value of property per pupil and in the number of one and two teacher schools, these inputs were still unequal by 1955. The value of school property was still 15 percent lower in black schools than white schools, and 15 percent of the black schools still had only one or two teachers compared to 4 percent for the white schools.

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[16] The *F*-statistic was 1.23, which has a marginal significance level of .3.

[17] These results are similar to the findings of Robert Margo's (1986) study of the effects of school quality on county illiteracy rates in Alabama between 1920 and 1940. Margo found that equalizing school quality would reduce the gap in literacy between blacks and whites aged 7 to 20 by an average of 27 percent, versus an average reduction in the achievement gap of 37 percent reported in Table 3. The somewhat larger effects of equalizing schools in this study are presumably attributable in part to the fact that Margo's literacy measure includes individuals who are not in school and therefore not affected by changes in school quality. His results would thus tend to understate the impact of school equalization on student achievement.

Average daily attendance also converged but was not equalized. By 1955, black attendance rates had risen to 92 percent versus 93 percent in the white schools. The results of Tables 2 and 3 indicate that this pattern of convergence in school characteristics across the racial systems in Maryland should correspond to a further convergence in school achievement across the racial systems in the 1940's and 1950's. While it is speculative to relate school achievements to market wages, the timing of the convergence in school characteristics in Maryland is consistent with the convergence in black-and-white returns to schooling reported by Smith and Welch (1986).

IV. Conclusion

This paper analyzed the factors underlying the observed differences in school attendance and achievement between black-and-white grade school children during the segregated era. It was shown that the production process for school achievement and the demand for school attendance were significantly affected by school quality. The results show that, had school quality been equalized at the level in existence in the white schools in the period, both attendance and human capital production in black schools would have increased substantially relative to the levels found in the white schools. These results are consistent with the view that schooling is an important factor in explaining the patterns of human capital accumulation and relative wages for blacks and whites.

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Farm Failures and Government Intervention: A Case Study of the 1930's

By RANDAL R. RUCKER AND LEE J. ALSTON*

Increasing farm failure rates since 1980 have given rise to demands for federal and state policies to alleviate agricultural distress. Three policy options that have been considered are increased subsidies under the existing commodity programs; subsidies and write-offs by the Farm Credit Systems; and state moratoria on farm foreclosures.

Because of a lack of data, no empirical analysis has been conducted of the likely effects of these programs on farm failure rates. The present, however, is not the first time that agriculture has suffered unusually high failure rates. During the 1920's and 1930's, farms failed at rates that have not since been approached (see Alston, 1983). Moreover, the interwar period witnessed the first extensive intervention into agricultural commodity and mortgage markets. Reported below are estimates of the effects on farm failure rates during the 1930's of 1) the commodity programs of the Agricultural Adjustment Administration (AAA), 2) the expanded role of the federal government in the agricultural credit market, and 3) moratorium legislation passed by twenty-five states in the early 1930's. These estimates indicate that each program was important in reducing farm failures and that, in total, they saved between 146,000 and 278,000 farms from failing.

I. A Model of Farm Failure Frequency[1]

Farm failure resulting from early termination of mortgage agreements can arise from either foreclosures or bankruptcies. A lender will choose to foreclose on a delinquent loan if the discounted expected net benefits from foreclosing are positive. Factors affecting the frequency of foreclosure include changes in expected farm earnings, government programs, and nominal interest rates.[2] A general increase in expected earnings or the initiation of a new farm-subsidizing government program increases the payment prospects of borrowers, thereby decreasing the frequency of foreclosure. An increase in interest rates raises the opportunity costs of funds tied up in delinquent loans, hence

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[1] Throughout this paper, the term "farm failures" denotes legal foreclosures, loss of farm as a result of bankruptcy, loss of title by default of contract, sales to avoid foreclosures, and surrender of title or other transfers to avoid foreclosure.

[2] From a theoretical perspective, variance and covariance effects between farm mortgages and other components of lenders' portfolios affect the risk of agricultural lending and hence affect the frequency of foreclosure. In our empirical analysis, we assume that the covariance between the rates of return on farm mortgages and lenders' portfolios is constant over the period of our sample.

increasing the benefits of foreclosing and the frequency of foreclosures.

A loan agreement may also be terminated if the borrower files for bankruptcy. Borrowers file for bankruptcy if they believe the discounted expected net gains from doing so are positive. The frequency of bankruptcy varies with changes in expected earnings, government policies, and interest rates. It may differ from the frequency of foreclosure because of differences between lenders' and borrowers' expectations of future earnings, and because of differences in the costs and benefits to lenders and borrowers of foreclosure and bankruptcy.

The farm failure process is dynamic for at least three reasons. First, earnings affect savings, which in turn influence borrowers' future abilities to meet mortgage payments. Second, even if current earnings and assets are not sufficient to allow a borrower to make his present mortgage payments, both parties may choose to extend the contract (rather than foreclosing or declaring bankruptcy) if they expect future income to be sufficient to meet past-due and future mortgage payments. The desire of lenders to protect their reputational capital suggests a third source of dynamics. During economic downturns, lenders' abilities to delay foreclosures (and protect the value of their goodwill capital) depend on their access to capital markets and on the flow of mortgage payments in previous periods.

II. Empirical Specification and Results

Our empirical analysis focuses on the effects of government relief programs on failure rates and on the dynamic relationship between failure rates and earnings. The directions of causation between government programs and farm failures are not one way. While federal credit programs and state-legislated moratoria may have affected the incidence of farm failures, they were also political responses to high levels of farm distress. To correct for simultaneity problems resulting from this joint dependence among failure rates, the activities of federal credit agencies, and state relief legislation, we estimate the following three-equation system:

$$
\begin{aligned}
(1)\quad FAIL_{it} = {} & \alpha_0 + \sum_{j=0}^{J} \Omega_j EARN_{it-j} \\
& + \sum_{k=1}^{K} \theta_k EARN01\ldots k_{it} + \alpha_1 \% FEDDEBT_{it} \\
& + \alpha_2 MOR_{it} + \alpha_3 POSTMOR_{it} + \alpha_4 \% FM30_i \\
& + \alpha_5 \% URB30_i + \alpha_6 (LV20/LV12)_i \\
& + \alpha_7 VAREARN_i + \alpha_8 (DEBT/VALUE)_{it} \\
& + \alpha_9 INTRATE_t + \alpha_{10} PPAID_t \\
& + \alpha_{11} UNEMP_t + u_{it};
\end{aligned}
$$

$$
\begin{aligned}
(2)\quad \% FEDDEBT_{it} = {} & \beta_0 + \beta_1 FAIL_{it} \\
& + \beta_2 FDRDUM_t \\
& + \beta_3 (FDRDUM_t)(FAIL_{it}) + v_{it};
\end{aligned}
$$

$$
\begin{aligned}
(3)\quad MOR_{it} = {} & \phi_0 + \phi_1 FAIL_{it} + \phi_2 \% FM30_i \\
& + \phi_3 \% FEDDEBT_{it} + \phi_4 \% AGINCOME_{it} + \varepsilon_{it}.
\end{aligned}
$$

Variable definitions and sources are presented in Table 1.[3] To estimate this system we use pooled time-series cross-sectional data from the forty-eight contiguous states for the period 1925–39. In equation (1) the dependent variable is $FAIL_{it}$, the annual rate of farm failure in state i during year t. Aggregate farm income varies across states with the absolute size and relative importance of the agricultural sectors. We adjust for such interstate differences by using farm earnings in state i during year t as a percentage of farm earnings in that state in a base period (1924–25) as our empirical proxy for earnings ($EARN_{it}$).[4]

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[3] Because our motivation for estimating equations (2) and (3) is to control for bias in equation (1), we shall not discuss their specifications or estimated coefficients. For more detail on these equations, see our earlier paper (1986).

[4] Because mortgage payments were fixed in nominal terms, we use nominal (rather than real) farm earnings in our empirical analysis.

TABLE 1—VARIABLE DEFINITIONS AND SOURCES

$FAIL_{it}$: Failure rate per 1000 farms in state i during year t (March 15 of year t–March 1 of year $t+1$). *Sources*: USDA, *The Farm Real Estate Situation*, annual issues, 1926–42.

$EARN_{i,t-k}$: Earnings (cash receipts from farm marketings and government payments) in year $t-k$ as a percentage of earnings in a base period (average of earnings in 1924 and 1925) in state i. *Source*: USDA, *Cash Receipts from Farming* (January 1946).

$EARN01\ldots k_{it}$: Interactive terms between $EARN_{it}$, $EARN_{it-1},\ldots, EARN_{it-k}$.

$\%FEDDEBT_{it}$: Percentage of total debt held by federal credit agencies in state i as of time t. *Source*: Harold Larsen (August 1945).

MOR_{it}: Dummy variable to indicate whether foreclosure moratorium legislation was in effect in state i in year t. This variable is assigned a value of 1 if such legislation was in effect for at least 6 months of a given year, and a value of 0 otherwise. *Source*: U.S. Congress (1936).

$POSTMOR_{it}$: Dummy variable assigned a value of 1 for two years following the expiration of moratorium legislation, and a value of 0 otherwise. *Source*: U.S. Congress (1936).

$\%FM30_i$: Percentage of farms in state i mortgaged in 1930. *Source*: U.S. Department of Commerce (1943, Table 17).

$\%URB30_i$: Percentage of population in state i that was urban in 1930. *Source*: U.S. Department of Commerce (1942, Table 8).

$LV20/LV12_i$: Ratio of indexes of land values (1920/1912) in state i. *Source*: M. M. Regan and A. R. Johnson (November 1942).

$VAREARN_i$: Variance of $EARN$ in state i for the period 1924–40.

$DEBT/VALUE_{it}$: Debt-to-value ratio in state i as of year t. Sources: Index of per acre land values is found in Regan and Johnson. An index of farm mortgage debt is constructed (base year = 1924) from data on farm mortgage debt found in Horton et al.

$INTRATE_t$: Prevailing rates on customers' prime commercial paper (4–6 months) in New York in year t. *Source*: *Federal Reserve Bulletin*, Vols. 11–25.

$PPAID_t$: National index of prices paid by farmers in year t for family living and production expenses, and interest, taxes and wages. *Source*: U.S. Department of Commerce (1975, p. 489).

$UNEMP_t$: U.S. annual average unemployment rate in year t. *Source*: U.S. Department of Commerce (1975).

$FDRDUM_t$: "Roosevelt dummy." This variable is assigned a value of 0 for years prior to 1933 and a value of 1 for 1933 and after.

$\%AGINCOME_{it}$: Proportion of total income from agriculture in state i in year t. *Source*: Frank Hanna (1959, pp. 28–29 and 248–49).

Because the dynamics of the farm failure process arise from all three of the sources discussed in Section I, we do not specify the form of the lag structure on earnings a priori. Instead, we estimate a free-form lag structure in which lagged earnings enter both directly ($EARN_{it-k}$) and through interactive terms ($EARN01_{it} = EARN_{it} \times EARN_{it-1}$, $EARN012_{it} = EARN_{it} \times EARN_{it-1} \times EARN_{it-2}$, etc.). The interactive terms are included because we expect the influence of a change in earnings in a particular year on farm failures in that year and future years to depend on the levels of earnings in neighboring years. For example, the effect of a change in earnings in year t on failure rates in year t will depend on the level of earnings in earlier years. If earnings in years $t-1$, $t-2$, etc., increase, debtors' stocks of savings will be larger (*ceteris paribus*), and the effect of a decrease in earnings on contemporaneous failure rates will be smaller. Similarly, the influence of earnings in year $t-1$ on failure rates in year t depends on the level of earnings in periods t, $t-1$, etc.

Insofar as the AAA programs achieved their objectives, their effects are captured indirectly by our earnings variable, which includes both cash receipts from farm marketings and government payments. We expect increased involvement in credit markets by federal credit agencies ($\%FEDDEBT$) to decrease farm failure rates. The effects on farm failures of state-legislated moratoria on mortgage foreclosures are measured with a dummy variable (MOR_{it}). If the moratoria simply delayed rather than prevented the failure of some farms, then an increase in farm failures would have followed expiration of the legislation ($POSTMOR_{it}$).[5]

Table 2 displays the coefficient estimates and summary statistics from our dynamic specification of the farm failure equation. The high-order lag on the earnings variable and the general significance of the mul-

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[5] In addition to earnings and government programs, we expect several other factors to influence farm failure rates. Space limitations preclude discussion of the expected signs or estimated coefficients of the other variables in equation (1). See our earlier paper and Alston for more detail on these variables.

TABLE 2—DETERMINANTS OF FARM FAILURES, 1929–39[a]

Independent Variable	Dependent Variable: $FAIL_{it}$ Coefficient Estimate	Asymptotic t-Ratio
Constant	40.57	1.63
$EARN_{it}$	−.378	−4.77
$EARN_{it-1}$	−.259	−3.72
$EARN_{it-2}$	.196	3.88
$EARN_{it-3}$	−.006	−.12
$EARN_{it-4}$	−.242	−5.13
$EARN01_{it}$	.005	4.00
$EARN012_{it}$	−2.7E−5	−2.45
$EARN0123_{it}$	−7.0E−8	−.74
$EARN01234_{it}$	1.1E−9	2.03
$\%FEDDEBT_{it}$	−.488	−4.05
MOR_{it}	−11.2	−3.18
$POSTMOR_{it}$	−6.24	−2.99
R^2	.771	
Sum of Squared Errors	14684	
Degrees of Freedom	498	
$\hat{\rho}^b$	.706 (.035)	
Mean of $FAIL_{it}$	19.52	

[a] The two-stage least squares regression estimates are obtained using an algorithm for estimating nonlinear simultaneous-equations systems. Regional dummy variables are also included in this equation to correct for possible omitted variable bias.

[b] Estimated coefficient (and standard error) of the first-order autoregressive disturbance.

tiplicative terms support our prior expectations that earnings influence failure rates in a complex fashion.[6] The coefficient estimates from this model suggest that the long-run effect on failure rates of a one-unit increase in *EARN* (evaluated at the sample mean of *EARN*) is to reduce annual failure rates by 0.335 per thousand farms.[7] A one-standard-deviation increase in *EARN* (from its mean) thus reduces the annual failure rate by 7.35 per thousand farms, a reduction of about 38 percent of the mean level of failure rates during this period. Similarly, the short-run (contemporaneous) effect of a one-unit increase in *EARN* is to reduce failure rates by 0.148 per thousand farms.[8]

Three conclusions can be drawn concerning the effects of the different government relief programs. First, to the extent that the AAA affected earnings, the estimated coefficients of the earnings variables suggest that (at least initially) the programs were successful.[9] Second, the estimated coefficient on $\%FEDDEBT_{it}$ is consistent with the hypothesis that the efforts of the federal credit agencies resulted in reduced farm failure rates. Finally, the estimated coefficient of MOR_{it} suggest that the state-legislated moratoria on foreclosures had the intended effect on farm failure rates. The coefficient of $POSTMOR_{it}$ does not support the suggestion that state relief legislation simply delayed foreclosures until the moratoria expired.

III. The Effects of Government Relief Programs

The regression coefficients in Table 2 can be used in combination with other contemporary information to estimate the magnitude of the effects of the government relief programs. Column 1 in Table 3 shows the preventative effects of the expanded role of federal credit agencies. For example, between 1934 and 1935, federal agencies increased their percentages of total mortgage debt by 15.9 percentage points. This induced an estimated decline in failure rates of 15.9 (.488) = 7.759 per thousand farms and pre-

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[6] These results differ from those of Lawrence Shepard and Robert Collins (1982). They estimated their model using national farm bankruptcy data and found that for the interwar period, their proxy for income did not significantly affect bankruptcy rates, and "the explanatory power of the equations was not significantly enhanced by further inclusion of lagged variables" (p. 612).

[7] To obtain this estimate, let E^* and F^* be the initial equilibrium levels of *EARN* and *FAIL*. Then $\partial F^*/\partial E^* = [-.378-.259+.196-.006-.242]+.01E^*-(8.1E-5)(E^*)^2-(2.8E-7)(E^*)^3+(5.5E-9)(E^*)^4 = -.335$ for $E^* = 76.41$ (the sample mean of *EARN*).

[8] To obtain this estimate, let $E_{it-k} = EARN_{it-k}$. From equation (1) and the coefficient estimates in Table 2, $\partial FAIL_{it}/\partial E_{it} = -.378+.005E_{it-1}-(2.7E-5)(E_{it-1})(E_{it-2})-(7.0E-8)(E_{it-1})(E_{it-2})(E_{it-3})+(1.1E-9)(E_{it-1})(E_{it-2})(E_{it-3})(E_{it-4}) = -.148$ for $E_{it-k} = 76.41$.

[9] Future research using data extending to the 1980's will examine the possibility that in the long run (after the value of program payments was capitalized into land values), the programs did not affect farm failure rates.

TABLE 3—ESTIMATED NUMBER OF FARM FAILURES PREVENTED BY GOVERNMENT RELIEF PROGRAMS, 1933–39

Year	Federal Credit Programs (1)	State Moratoria Max (2a)	State Moratoria Min (2b)	AAA Programs Max (3a)	AAA Programs Min (3b)	Total Max (4a)	Total Min (4b)
1933	1,139	28,630	28,630	4,029	4,029	33,798	33,798
1934	9,900	34,433	5,332	13,367	9,338	57,700	24,570
1935	47,300	27,735	6,984	16,339	2,972	91,374	57,256
1936	15,485	23,069	–	7,665	0	46,219	15,485
1937	4,880	4,638	–	8,820	1,155	18,338	6,035
1938	566	1,279	–	10,851	2,031	12,696	2,597
1939	−2,209	–	–	19,709	8,858	17,500	6,649
Total	77,061	119,784	40,946	80,780	28,383	277,625	146,390

Notes: Col. 1: $_t = [\%FEDDEBT_t - \%FEDDEBT_{t-1}] \times [\partial FALL/\partial \%FEDDEBT] \times$[number of farms in the United States in year t/1000]. *Sources*: $\%FEDDEBT$; see Table 1. $\partial FAIL/\partial \%FEDDEBT = -.488$; coefficient estimate from Table 2. Number of farms in the United States; source for 1935 is the *U.S. Census of Agriculture*. Numbers for noncensus years are estimated by linear interpolation between census years. To better approximate the number of farms that could be mortgaged, we delete the number of farms operated by sharecroppers from the census figures (as first suggested by E. H. Wiecking, October 1927, p. 35).

Col. 2a: $_t =$ [number of farms in states with moratoria in effect in year t/1000]$\times[\partial FAIL/\partial MOR]$. *Sources*: States instituting moratoria; see source for MOR_{it} in Table 1. Number of farms in states with moratoria in effect; *U.S. Census of Agriculture*. $\partial FAIL/\partial MOR = -11.2$; coefficient estimate from Table 2.

Col. 2b: $_t =$ [number of farms in states instituting moratoria in year t/1000]$\times[\partial \text{FAIL}/\partial \text{MOR}]$. *Sources*: same as col. 2a.

Col. 3a: $_t =$ [change in $EARN$ due to AAA in year t]$\times[\partial FAIL/\partial EARN]\times$[number of farms in the United States in year t/1000]. *Sources*: $EARN$; see Table 1. Change in $EARN$ due to AAA in year t is calculated under the assumption that the change in income due to AAA programs was the level of government payments in year t multiplied by 1.58/1.15 (see text). $\partial FAIL/\partial EARN$; estimated regression coefficients from Table 2 are used to calculate these long-run dynamic effects (see fn. 6). The presence of the interactive earnings terms necessitates some assumption concerning the "long-run equilibrium" levels of $EARN_t$. For the calculations in this table, we assume that the equilibrium value of $EARN_t$ was $EARN_{t-1}$.

Col. 3b: $_t = (\text{col. 3a})_t - (\text{col. 3a})_{t-1}$ if $(\text{col. 3a})_t - (\text{col. 3a})_{t-1} > 0$, and $= 0$ otherwise.

Col. 4a: $_t = (\text{col. 1})_t = (\text{col. 2a})_t + (\text{col. 3a})_t$.

Col. 4b: $_t = (\text{col. 1})_t + (\text{col. 2b})_t + (\text{col. 3b})_t$.

vented $7.759\,(6{,}096{,}094/1000) = 47{,}300$ farms from failing.[10]

Whereas a mortgage issued by a federal credit agency provided relief to the same farmer year after year, a given flow of relief from state moratoria and from AAA programs may have prevented different farms from failing in different years. To provide upper and lower bounds on the number of farms saved by state moratoria and AAA programs, we use two extreme assumptions: they prevented the same farms from failing year after year; and they prevented a completely different set of farms from failing each year.

Columns 2a and 2b in Table 3 present our minimum and maximum estimates of the effects of state moratorium legislation. The resulting lower and upper bounds on the estimated total number of farms saved from failing by moratoria are 40,946 and 119,784.

Columns 3a and 3b provide estimates of the effects of the AAA programs. To approximate the impact of these programs on farm failures, we use the estimated effects of AAA programs on farm revenues developed by Edwin Nourse et al. (1937, p. 323). They estimated that between 1933 and 1935, AAA programs increased gross farm income by about $1.58 billion. During the same period, total direct government payments were $1.15 billion. Although data on direct government payments are available for the period

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[10] In this calculation we assume that the mortgage loans issued by federal credit agencies during the period 1933–39 did not terminate until after 1939. See Donald Horton et al. (1942, pp. 102–04 and 121–22) for figures documenting the low annual turnover from failure on federal loans during this period.

1936–40, no estimates of the effects of AAA-type programs on farm incomes exist for those years. To approximate the effect of AAA programs, we assume that the ratio of government payments to changes in farm incomes due to farm programs from 1936 to 1939 was the same as that during 1933–35 (1.58/1.15). Our upper-bound estimates indicate that the total number of farms saved from failure by AAA programs between 1933 and 1939 is 80,780. Our lower-bound estimate is 28,383, the sum of the entries in column 3b (which are the positive increments in col. 3a).[11]

The estimates developed in Table 3 suggest that the government relief programs saved between 146,390 and 277,625 farms during the 1930's. We suspect that there is relatively little temporal movement of farmers within the income rankings (i.e., the same farmers have trouble meeting their mortgage payments year after year), so the actual number of farms saved by these programs probably is closer to the lower estimate. If, for example, one assumes 70 percent of the farms prevented from failing by relief from the AAA or state moratoria in a particular year were also prevented from failing in the following year, then a rough approximation of the number of farms saved is 186,000.[12]

IV. Conclusions

Our empirical results are consistent with the hypothesis that government programs successfully alleviated farm distress during the 1930's. Before our estimates are used to assess the welfare implications of government intervention, however, several issues must be considered. First, in the absence of a model of the optimal level of farm failures, we cannot determine whether government relief programs of the Great Depression corrected a "market failure" or interfered with properly functioning market processes. Second, we have not determined whether the government programs instituted in the 1930's yielded benefits extending beyond that decade. Third, although measuring the costs of intervention is beyond the scope of this paper, it is possible to indicate the sorts of costs imposed on various parties.

State moratorium legislation imposed costs on lenders by restricting their abilities to exercise an option originally included in their contracts with borrowers. Low interest rate loans and principal reductions by federal credit agencies were subsidized by the U.S. Treasury, thereby imposing costs on taxpayers. Finally, the programs initiated by the AAA had both direct costs and less apparent efficiency costs. In all likelihood, the long-run costs associated with the AAA dwarfed the costs of the other programs.

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[11] This number represents the total number of different farms saved from failing for at least one year by AAA programs. A more meaningful figure may be the number of farms saved from failing throughout the period 1933–39. This would be the minimum of the entries in col. 3a, or 4,029 farms.

[12] We obtain this figure by interpolating between the high and low estimates for the effects of AAA programs and moratoria and then summing across all three programs. That is, $186,000 \approx 77,061 + [40,946 + .3(119,784 - 40,946)] + [28,383 + .3(80,780 - 28,383)]$.

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Is the Concentration-Profit Correlation Partly an Artifact of Lumpy Technology?

By VAL EUGENE LAMBSON*

Much effort has gone into establishing a relationship between market concentration and profit rates. Most authors now seem to agree that a weak but positive correlation exists. The traditional interpretation of this correlation is that market concentration fosters collusion, resulting in monopoly rents. (See Leonard Weiss, 1974.) On this basis some have argued that the government should break up large firms, thus reducing concentration and making collusive behavior less likely.

The traditional interpretation came under fire from economists who argued that it ignores the possibility of entry. These economists developed the "efficient structure hypothesis" which asserts that concentration results because more efficient firms are able to capture larger market shares and Ricardian rents. (See Harold Demsetz, 1974.) Although this hypothesis does not rule out collusion by the more efficient firms, it does yield a different policy prescription. To quote Demsetz,

> You can rationalize some of my data in the following way: in concentrated industries the cost to the larger firms is lower. That is why their profit rates tend to be higher than for the smaller firms in these industries.
>
> Let us suppose that the larger firms in concentrated industries are colluding and that is what keeps the price up. But the collusion keeps price at a low enough level that the next size firms do not make higher profit rates in concentrated industries than they do in unconcentrated industries.
>
> Therefore, the price that is set by the large firms in the concentrated industries is approximately at a level that just covers the cost of the next-size-down firms in those industries. If you break apart the large firms in those industries and convert them into next-size-down firms, you will not lower price at all. [p. 244]

Demsetz mentions three characteristics: asymmetric technology (some firms are more efficient); collusion; and scale economies (if a large firm is broken up, the resulting firms are less efficient). It is natural to wonder what part each characteristic might play in the observed concentration-profit correlation. In what follows, attention is principally focused on the effects of scale economies.

Consider a hypothetical data set made up of industries where firms can enter upon the investment of a fixed cost. Assume that all firms in an industry have access to the same technology. Focus on a single industry where firms must invest F to enter. If N firms enter, assume some symmetric equilibrium (for example, Cournot) and let π_N be the net operating profit (before F is deducted) accruing to a firm in equilibrium. If, as is usually the case, π_N is decreasing in the number of firms then an equilibrium number of firms N^* can be defined to satisfy

$$(1) \qquad \pi_{N^*} \geq F \geq \pi_{N^*+1}.$$

Now define the rate of profit as the equilibrium ratio of net operating profit to fixed costs (i.e., $R \equiv \pi_{N^*}/F$). (A multi-period model where profit rates are measured by the ratios of discounted operating profits to fixed cost could easily be used without changing the results.) Economic profits are made if this ratio exceeds one. Since in general $[\pi_{N^*+1}, \pi_{N^*}]$ will be an interval of positive length, R will almost always exceed one; its magnitude will depend on where F lies on the interval. Straightforward manipulation of (1) yields

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$$(2) \qquad 1 \le R \le \pi_{N^*}/\pi_{N^*+1} \equiv R_{N^*},$$

so R_{N^*} puts a limit on how far R can deviate from one. Now suppose the data set consists of m such industries. Since all equilibria are symmetric, the s-firm concentration ratio is simply s/N^*, so concentration and N^* are negatively correlated in the sample. Hence if R_{N^*} (unobserved) is negatively correlated with N^* across the industries, then a positive correlation between R and concentration will result unless, by chance, the sample is sufficiently skewed in the direction that R lies near one in industries with small N^* and near R_{N^*} in industries with large N^*. The point can be made concretely with an example.

Suppose, for each k, the kth industry in the data set faces a linear inverse demand function $f_k(Q) = \alpha_k - \beta_k Q$, where Q is total industry output while α_k and β_k are positive constants. Each firm in the kth industry must invest F_k if it decides to produce, after which it can produce any quantity q at constant marginal cost γ_k. Then the operating profit of an individual firm in industry k (given N firms) is

$$(3) \qquad \pi_N^k(q, \hat{Q}) = [\alpha_k - \beta_k Q]q - \gamma_k q,$$

where $\hat{Q}$ is the sum of the other firms' outputs in the industry (i.e., $q + \hat{Q} = Q$). Assume that firms behave as Cournot competitors, so in equilibrium (given N active firms) each firm will produce q^c satisfying

$$(4) \qquad \pi_N^k(q^c, \hat{Q}^c) = \max{}_q \pi_N^k(q, \hat{Q}^c),$$

where $\hat{Q}^c = (N-1)q^c$. The usual method of calculation verifies that

$$(5) \qquad \pi_N \equiv \pi_N^k(q^c, \hat{Q}^c) = (\alpha_k - \gamma_k)^2/\beta_k(N+1)^2.$$

So if $N^*(k)$ is the equilibrium number of firms in industry k, then

$$(6) \qquad R_{N^*(k)} \equiv \pi_{N^*(k)}/\pi_{N^*(k)+1} = [N^*(k)+2]^2/[N^*(k)+1]^2.$$

A simplifying artifact of the linear structure is that (6) is independent of demand and cost parameters. So for any industry in the data set that has N^* firms, R will satisfy (from (2) with the appropriate substitution)

$$(7) \qquad 1 \le R \le (N^*+2)^2/(N^*+1)^2 \equiv R_{N^*}.$$

Note that (7) exhibits the negative correlation between R_{N^*} and N^* alluded to previously. The subsample of the data set composed of N^*-firm industries will exhibit a distribution of profit rates (R) over the interval defined by (7). Since the maximal profit rates (R_{N^*}) that can arise in industries with small equilibrium numbers of firms (high concentration) exceed those that can arise in industries with large equilibrium numbers of firms (low concentration) there is a presumption that profit rates and concentration will be positively correlated. Furthermore, since $R_{N^*} - R_{N^*+1}$ is decreasing in N^*, the correlation will tend to be weaker if the data set contains many industries with low concentration. Both of these outcomes are consistent with empirical findings.

The negative correlation between R_{N^*} and N^* that occurs in linear Cournot models appears in many other models as well. Using similar reasoning, Donald Dewey (1976) showed that when identical firms collude while enjoying scale economies, a positive correlation between concentration and profit is to be presumed.[1] In general, as is clear from (2), a positive concentration-profit correlation is to be presumed if π_{N^*}/π_{N^*+1} decreases in N, that is, if the *percentage*

[1] Actually, Dewey employed the Chamberlin model of oligopoly which, however, results in maximal profits for identical firms. Modern treatments of collusion imply maximal profits for identical firms only under certain conditions. See, for example, Dilip Abreu (1986), Abreu et al. (1986), James Friedman (1977), and my paper (1984).

change in net operating profit has smaller absolute value as N is larger. This condition, which is necessary and sufficient, holds in a wide variety of the standard oligopoly models.

In summary, the larger are scale economies, the higher industry profit rates can be without being depressed by entry. This is because entry is more likely to depress profit rates to negative levels when scale economies are more significant. Hence entry stops short of lowering profit rates to normal levels, and it stops short by greater amounts when scale economies are greater.

A final comment is in order. Although lumpy technology can explain part of the correlation between concentration and profit, it is difficult to argue that all of the correlation can be explained in this manner. As pointed out by the referee, in most industries enough output is produced by small- and medium-size firms to accommodate at least one firm of a size equal to or larger than one or more of the largest four firms in the industries. (Four-firm concentration ratios rarely exceed 80 percent.) Furthermore, no attempt was made above to explain the large differences in size that usually exist between the largest firm in concentrated industries and the next largest firms. Finally, the largest firm in concentrated industries often earns a higher rate of profit than its competitors. All of this is consistent with the larger firms having achieved their success on the basis of superior performance, indicating that the characteristic ignored throughout, namely asymmetric technology, is also behind the concentration-profit correlation. This paper simply establishes that even if all firms were identical and did not collude, one would still expect a positive correlation between concentration and profit.

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Public Utility Pricing under Risk; The Case of Self-Rationing: Comment and Extension

By PETER M. SCHWARZ AND THOMAS N. TAYLOR*

In their article in this *Review* (1978), John Panzar and David Sibley (P-S) offer a model of electricity load management with uncertainty. Unlike other schemes where the utility rations load under excess demand, consumers ration themselves by purchasing a fuse. Our purpose is twofold. First we generalize two assumptions implicit in the P-S article. We allow individual customer demand to fluctuate within a billing period. Also, we allow diversity or imperfect correlation among customer demands. Panzar and Sibley imply that power demand is uniform over the period, and that all consumer demands are identically correlated to a central variable, temperature.

Second, we extract the behavioral effects of a fuse that assist in understanding a demand charge. A demand charge assesses individual customer maximum demand. It is our contention that the modified fuse model is superior to existing models as a representation of the demand charge.[1] Much of the literature converts the demand charge into an energy-equivalent charge by allocating it proportionately to each hour of the billing period.[2] The behavioral implication is that the demand price is effective at each instant of the billing period. The fuse analogy suggests that demand and energy prices be kept separate, and that behavior is best understood as a two-stage procedure. First, an energy load curve is chosen as if only the energy charge were in effect. This curve is then modified and constrained due to the cost of maximum demand.

In Section I, we present terminology that facilitates generalization of the P-S model. In Section II, we allow for nonuniform individual customer demands, and in Section III, diversity among demands.

I. Terminology

Energy (E) is a measure of the ability to perform work. It is the product of power (q) and time (n). Power is the instantaneous rate of energy consumption and is measured in kilowatts (kw). Time is measured in hours, so energy is in kilowatt-hours (kwh); $E = \int_{n=0}^{N} q\,dn$, for a period from time 0 to time N.

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[1] The emerging literature on U.S. experience includes our earlier paper (1986), Dennis Aigner and Joseph Hirschberg (1985), A. Ronald Gallant and Roger Koenker (1983), Douglas Caves et al. (1984), Schwarz (1984), Michael Veall (1983), J. Stephen Henderson (1983), and Taylor (1982). There is also a wealth of literature on European demand rates. Veall (footnote, p. 427) recognizes similarities between the fuse of P-S and a demand charge, but does not explore this.

[2] Aigner-Hirschberg, Caves et al., Lee Lillard and Aigner (1983), and Andreas Savvides (1983) model time-of-day use as a response to an energy-equivalent rate. The demand charge is divided by the length of the billing period and added to the energy price. While the demand charge is initially entered as a separate price in the budget constraint, and in some models capacity (maximum demand) is a separate argument in the utility function, the implication is that consumers react to demand charges as they would the energy-equivalent rate. In essence, the demand charge is effective at each instant of the peak period. An alternative view can be found in Jorg Finsinger and Paul Kleindorfer (1981), Gallant-Koenker, Henderson, and our earlier paper, that leaves the rates unbundled. The differing behavioral implication is that the demand charge is effective at reducing the customer's own maximum demand, but its effects at other times, including system peak, are empirical matters.

A fuse is priced in dollars per kw, assessed to a subscribed maximum level of power to be consumed over a billing period. This subscribed maximum is referred to as fuse capacity. Billing periods may vary but typically cover 30 days. In the electric utility industry, the terms "demand" and "power" are used interchangeably. A demand charge is also a dollar/kw charge assessed to the maximum level of power or demand realized during a billing period. Current modeling efforts treat demand as an *ex ante*, rather than *ex post* quantity. That is, price effects reflect consumer planning for a maximum level of power to be consumed during the billing period. An energy charge is a cents/kwh charge, and assesses the integral of power over the billing period.

Panzar and Sibley do not elaborate on the time period for which the fuse is purchased. By assuming uniform power demand over the period, it is not necessary to incorporate time, nor to distinguish between power and energy. Incorporating time is a minor modification for stage 1 determination of energy use in the absence of a fuse charge. However, the second stage necessitates treating the fuse as a quasi-public good, rather than a private good.[3] Figure 1 is an example of the determination of energy and fuse capacity for a billing period.

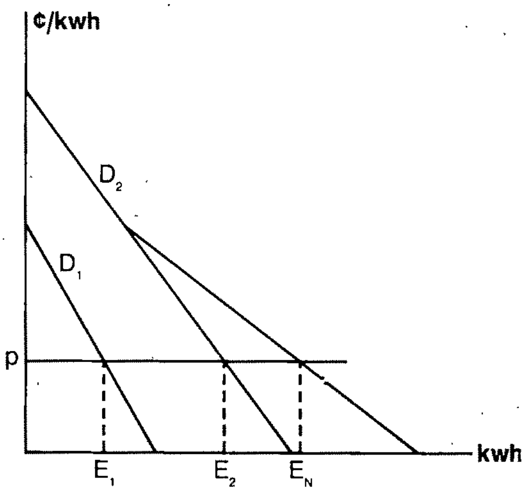
For ease of interpretation, assume the demand for power is q_1 for the first half of the billing period and a higher level q_2 for the second half. Then,

$$D_1 = \int_{n=0}^{.5N} q_1\, dn = \frac{N}{2} q_1$$

and $$D_2 = \int_{n=.5N}^{N} q_2\, dn = \frac{N}{2} q_2.$$

Stage 1 determination of energy consumption at price p is $E1$ for the first half of the billing period and E2 for the second half.

Stage 1: The Demand for Energy (E)



Stage 2: The Demand for Fuse Capacity (A)

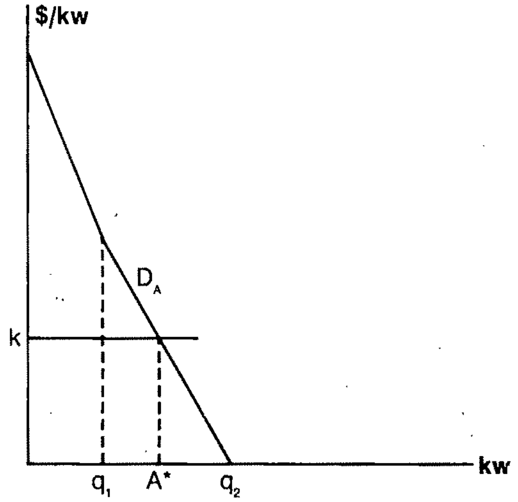


FIGURE 1

Total energy consumption is

$$E_N = E_1 + E_2 = \frac{N}{2} q_1 + \frac{N}{2} q_2.$$

Energy is an ordinary private good, so that billing-period energy is a horizontal summation of E_n.

Having determined optimal *ex post* (P-S terminology) energy consumption, the consumer now turns to optimal fuse purchase. The fuse is a quasi-public good in that usage at one instant does not reduce its availability at other points in the billing period. Hence, willingness to pay for the fuse is the vertical sum of *excess* willingness to pay for the two energy demands. The first q_1 units of power have marginal benefits throughout the entire billing period, so that the demand for power, D_A, from 0 to q_1 is the vertical sum of $(D_1 - p)$ and $(D_2 - p)$. Between q_1 and q_2, power has value only for the second half of the billing period, so D_A reflects only the marginal benefits of D_2. Optimal fuse capacity is A^* at price k. Because power is constant in the P-S model, the public good aspect of the fuse is absent.

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[3] The P-S treatment of the fuse does not parallel an earlier work of Panzar (1976), wherein capital input k was modeled as a public input.

II. Consumer Behavior for a Billing Period

Consumer utility in Panzar and Sibley is a functional form for which maximization of consumers' surplus is equivalent to utility maximization. The form is

$$u(q,t,\theta) = \int_0^q P(q',t,\theta)\,dq'$$

where q = quantity of power consumed; t = a random variable such as temperature; θ = index of consumer types; and $P(q,t,\theta)$ is the marginal willingness-to-pay function (or the inverse demand function) of a consumer of type θ.

The purchased commodity, power, is defined as the rate of energy consumption. Their equation (4) for expected consumers' surplus provides a weighting by the probability of states of nature for a single period:

$$\text{(1)}\quad ES = \int_{t=t_L}^{\hat{t}}\left[\int_{q=0}^{q^*} P\,dq - pq^*\right] f(t)\,dt + \int_{\hat{t}}^{t_H}\left[\int_{q=0}^{A} P\,dq - pA\right] f(t)\,dt - kA$$

where $\hat{t}$ = the temperature at which the fuse constrains desired demand (q^*), t_L and t_H are the limits of the distribution; A = fuse size, k = price per unit of fuse, and p = price per unit of power.

Optimal fuse size (A^*) is obtained by differentiating the P-S equation (4) (see equation (5) of P-S):

$$\text{(2)}\quad \partial ES/\partial A = \int_{\hat{t}}^{t_H}[P(A^*,t,\theta) - p]\,f(t)\,dt - k \leqq 0.$$

Panzar and Sibley interpret this result: "... the consumer sets A^* at the level for which the price of an increase in fuse size k equals the expected excess of willingness to pay over the price of consumption p" (p. 890).

Over a billing period, the temperature distribution and the demand for power will vary with the result that the fuse will likely constrain power consumption in more than one period, and at some instants within a period when demand fluctuates. This result leads to a greater "expected excess of willingness to pay" than is incorporated in equation (1). Consequently, the optimal fuse size is larger than in P-S. A proof of this intuition follows subsequent to equation (5).

We begin by explicitly defining the consumer utility function at an instant

$$u(q,t,n,\theta) = \int_0^{q_n} P(q',t,n,\theta)\,dq'.$$

Then for the billing period:

$$u(q,t,\theta) = \int_{n=0}^{N}\int_0^{q'} P(q',t,n,\theta)\,dq'\,dn.$$

The *ex post* desired demand function is obtained by maximizing consumers' surplus over the billing period:

$$\max_{|q \geqq 0|} \int_0^N \int_0^{q_n} P(q',t,n,\theta)\,dq'\,dn - p\int_0^N q(t,n,\theta)\,dn.$$

At each instant there exists a state of nature $\hat{t}_n$ such that desired demand just equals fuse capacity:

$$\text{(3)}\qquad q_n^*(p,t_n,\theta) = A.$$

Expected surplus is

$$(4)\quad ES=\int_0^N\left\{\int_{t_{Ln}}^{\hat{t}_n}\left[\int_0^{q_n^*}P_n\,dq_n-pq_n^*\right]\times f(t_n)\,dt_n\right\}dn+\int_0^N\left\{\int_{\hat{t}_n}^{t_{Hn}}\left[\int_0^{A}P_n\,dq_n-pA\right]\times f(t_n)\,dt_n\right\}dn-kA.$$

From equation (3) the implicit function for $\hat{t}$ is now time-specific $\hat{t}_n(p,A,\theta)$. Optimal fuse capacity is

$$(5)\quad \partial ES/\partial A=\int_0^N\left\{\int_{\hat{t}_n}^{t_{Hn}}[P_n(A^*,t_n,\theta)-p]\times f(t_n)\,dt_n\right\}dn-k\leqq 0.$$

Comparing (5) and (2) for a given A^*,

$$\int_0^N\left\{\int_{\hat{t}_n}^{t_{Hn}}[P_n(A^*,t_n,\theta)-p]\,f(t_n)\,dt_n\right\}dn>\int_{\hat{t}}^{t_H}[P(A^*,t,\theta)-p]\,f(t)\,dt.$$

Hence, A^* as determined by (5) must be greater than A^* as determined by (2) proving that optimal fuse is larger than implied in Panzar and Sibley.

This altered condition leads to a policy implication not revealed by Panzar and Sibley: A fuse will be particularly effective in suppressing needle peaks in demand. For needle peaks, the duration of extreme temperature conditions is short leading to a relatively small value for the integral over time in (5) and a low optimal purchase of fuse capacity by the household. Hence, utilities planning for the use of a fuse to suppress maximum system demand might consider the nature of peaking conditions for their service area. If needle peaks are common, the fuse may be advantageous. The effect of the fuse is diluted, however, if peaking conditions derive from long periods of temperature extremes.

III. Optimal Pricing

The approach of Panzar and Sibley is to find values of p and k that maximize the sum of consumers' expected surplus (ES) plus expected profits ($E\pi$). Power is produced at a constant cost b per kilowatt hour, and capacity (in kilowatts) costs β per unit installed.

We allow diversity or imperfect correlation among customer demands. That is, we specify that the firm be committed to install capacity sufficient to cover its own maximum demand, Q, and not the total of maximum demands contracted for by each consumer.[4] For P-S, expected profits are given by their equation (7):

$$(6)\quad E\pi=\int_{\theta=\hat{\theta}}^{\theta_H}(p-b)\times\left[\int_{t=t_L}^{\hat{t}}q^*(p,t,\theta)f(t)\,dt+A^*\cdot[1-F(\hat{t})]\right]g(\theta)\,d\theta+(k-\beta)$$

where $S=\int_{\theta=\hat{\theta}}^{\theta_H}A^*(p,k,\theta)g(\theta)\,d\theta$ is the sum of individual fuse purchases.

Total expected profit is the sum of expected profits from each individual (θ) with consumption of $q^*(p,t,\theta)$ for $\hat{t}<t(\theta)$ and A^* for $t\geq\hat{t}(\theta)$. Capacity related revenues and costs are directly proportional to the sum of subscribed fuses S.

Jorg Finsinger and Paul Kleindorfer (1981) solve for p and k in a deterministic framework. Following them, we recognize that capacity revenue is directly proportional to S, while cost is dependent on system capacity Q which may be less than S.

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[4] System capacity Q in our model is a function of the temperature distributions at each point in time. Veall models capacity as a function of the Extreme Value (Gumbel) Distribution, proposing that Q be predicted based on a distribution of maximum values.

This specification leads to

$$(7)\quad E\pi = \int_{\theta=\hat{\theta}}^{\theta_H} \left\{ \int_{n=0}^{N} (p-b) \times \left[\int_{t=t_{Ln}}^{\hat{t}_n(\theta)} q_n^*(p,t,\theta) f(t_n)\, dt_n + A^* \cdot [1-F(\hat{t}_n)] \right] dn \right\} g(\theta)\, d\theta + kS - \beta Q + \int_0^N \mu_n (Q - Q_n)\, dn$$

where

$$Q_n = \int_{\hat{\theta}}^{\theta_H} \left\{ \int_{t_{Ln}}^{\hat{t}_n(\theta)} q_n^*(p,t,\theta) f(t_n)\, dt_n + A^*(p,k,\theta)[1-F(\hat{t}_n)] \right\} g(\theta)\, d\theta$$

is expected demand at time n.

Capacity costs are equal to $\beta Q_{\max}$ where $Q_{\max}$ is the maximum value of Q_n. As Finsinger and Kleindorfer note, rather than including $\beta Q_{\max}$ in (7), an equivalent expression incorporates a fixed level of capacity Q with the constraint $Q \geqq Q_n$. In addition, the Lagrange multipliers μ_n of (7) must sum to β. Maximizing expected welfare by differentiating with respect to p and k and using simplifications analogous to those of P-S (equation (9)):

$$(8)\quad \max_{\{p \geqq 0,\, k \geqq 0\}} EW = \int_{\theta=\hat{\theta}}^{\theta_H} [ES^*(p,k,\theta) + E\pi(p,k,\theta)]\, g(\theta)\, d\theta,$$

where ES^* is given by equation (4) with $A = A^*$:

$$(9)\quad \partial EW/\partial p = \int_{\theta=\hat{\theta}}^{\theta_H} \left\{ \int_{n=0}^{N} [p-b] \times \left[\int_{t=t_{Ln}}^{\hat{t}_n(\theta)} (\partial q_n^*/\partial p) f(t_n)\, dt_n + \partial A^*/\partial p [1-F(\hat{t}_n)] \right] dn \right\} g(\theta)\, d\theta + k\, \partial S/\partial p - \int_0^N \mu_n (\partial Q/\partial p)\, dn \leqq 0;$$

$$p \geqq 0, \quad p \cdot \partial EW/\partial p = 0.$$

$$(10)\quad \partial EW/\partial k = \int_{\theta=\hat{\theta}}^{\theta_H} \left\{ \int_{n=0}^{N} (p-b) \times \left[(\partial A^*/\partial k)[1-F(\hat{t}_n)] \right] dn \right\} g(\theta)\, d\theta + k\, \partial S/\partial k - \int_0^N \mu_n (\partial Q_n/\partial k)\, dn \leqq 0;$$

$$k \geqq 0, \quad k \cdot \partial EW/\partial k = 0.$$

Common to equations (9) and (10) are the conditions:

$$\mu_n \geqq 0, \quad \mu_n [Q - Q_n(p)] = 0; \quad \int_{n=0}^{N} \mu_n = \beta.$$

Kuhn-Tucker conditions are satisfied if $p = b$ and $k = [\int_0^N \mu_n (\partial Q_n/\partial k)\, dn]/\partial S/\partial k$, or $k = \int_0^N \mu_n (\partial Q_n/\partial S)\, dn$.

To interpret the optimality condition for k, the term $\partial Q_n/\partial S$ can be considered a proportionality factor relating the change in constrained system load at any time to a change in aggregate demand for fuse capacity. If this proportionality factor is equal to one, then $k = \beta$. This result follows from $\int_{n=1}^{N} \mu_n = \beta$. In the case considered by P-S, where customer demands are coincident, this result would hold.

If the proportionality factor is a constant C (not necessarily 1): $k = C \int_{n=1}^{N} \mu_n\, dn = C\beta$. For $C < 1$, $k < \beta$ and for $C > 1$, $k > \beta$.

Empirically, $C < 1$ is most plausible. Because of demand diversity among households, an increase in fuse purchases is likely to increase Q_n by a smaller amount. The case $C > 1$ implies that a marginal increase in fuse purchases causes a greater marginal increase in coincident demand. This case is theoretically possible, but intuitively not likely. If the proportionality factor is not a constant, no simple optimality condition for k can be obtained. Thus, the general result is that a single k would not be optimal. This outcome illustrates a practical difficulty in achieving optimal behavior with a demand charge or fuse. System peak demand (Q) influences cost, but information on cost is conveyed to the consumer by pricing a different quantity (A). Only if Q and A are related in a particular manner will optimality be achieved.

IV. Summary and Conclusions

Panzar and Sibley introduce a model of electricity rationing that requires the customer to purchase a fuse based on expected individual maximum demand. Implicit in their model is that power demand is uniform over the period. In deriving optimal prices per unit of fuse and power, customer demands are perfectly correlated with a common variable, temperature.

We generalize the model by allowing for nonuniform demands and diversity among demands. This leads to larger fuse purchases by individuals. The most likely effect on the system is that optimal capacity is less than, rather than equal to, the sum of individual fuse capacities.

The joint power-fuse purchase decision is behaviorally identical to current modeling of the energy-demand decision when a customer pays a two-part rate. Determination of energy employs the private goods condition of equating price and marginal benefit. Determination of demand, however, follows a quasi-public goods condition that price equal the sum of marginal benefits. Our work supports models that keep demand and energy prices separate, but generalizes these models by including uncertainty.

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Auctions with Contingent Payments: Comment

By William Samuelson*

In a recent paper in this *Review* (1985), Robert Hansen demonstrates that an auction allowing for contingent payments yields a greater expected revenue to the seller than an auction with fixed price bids. This "competitive" benefit is a potential explanation for why contingent pricing schemes are common in actual practice, where examples range from corporate acquisitions via exchange of securities, to revenue sharing in oil lease auctions, and incentive contracts in defense procurement.[1] This comment extends Hansen's analysis by noting several limitations of such contingent price schemes and emphasizing significant problems of adverse selection (an inefficient firm may win the auction) accompanying their use. It is shown that there is a basic tradeoff between the competitive benefits and the selection costs of increasing the extent of contingent payment in the auctioned contract.

In Hansen's model, a single seller faces a number of competing buyers who bid for the right to obtain an item. Let v_i denote the value of the item to firm i, and, following Hansen, suppose that these values are independently and identically distributed according to the probability distribution $F(v)$. If bidding is limited to fixed prices, then any of the standard auctions, sealed high bid or English, yields an expected revenue to the seller equal to the expectation of the second-highest value among the bidders, $E[R]=E[v_2]$. Now consider instead an auction based on profit share bids: the bidder allowing the seller to retain the greatest percentage profit share wins the auction. As Hansen shows, this contingent pricing scheme increases the seller's expected revenue, $E[R'] > E[R]$.

Somewhat hidden in Hansen's demonstration is the origin of the contingent payment's advantage. As Jacques Crémer points out in his comment (1987), the source is easily seen by considering an auction where the seller predetermines the profit share that the winning firm will acquire. Call the firm's share s; the seller retains share $1-s$. Given this sharing rate, firms submit "bonus" bids, and the highest bidding firm wins the auction. For convenience, attention is restricted to a sealed second-bid auction—a member of a wide class of optimal (i.e., revenue-maximizing) auctions. In this auction, the high bidder pays the seller a price equal to the second-highest bid. Define the reservation price of firm i as the value of its retained profit share, $r_i = sv_i$. It is well-known that each firm's dominant strategy is to bid its reservation price, $b_i = sv_i$. Henceforth, this auction will be referred to as an English auction since it is strategically equivalent to the common auction of that name. Let the bids be ordered $b_1 > b_2 \ldots > b_n$, and let $v_{(i)}$ denote the ith highest value among the n firms.

In equilibrium, the seller receives expected revenue

$$
\begin{aligned}
(1)\quad E[R] &= E[b_2]+(1-s)E[v_1] \\
&= sE[v_{(2)}]+(1-s)E[v_{(1)}] \\
&= E[v_{(1)}]-sE[v_{(1)}-v_{(2)}].
\end{aligned}
$$

It follows immediately that it is optimal for the seller to set $s=\varepsilon$, where ε is in the positive neighborhood of zero.[2] The seller obtains a near 100 percent share of the highest value among the competing bidders —that is, it obtains the first-best level of expected revenue, $E[R]=v_{(1)}$.

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[1] This general point has been highlighted recently by Paul Milgrom (1985). M. S. Robinson (1984) presents an in-depth discussion of the potential benefits and limitations of contingent contracts in oil lease auctions.

[2] Strictly speaking, the seller's problem has no optimum due to the discontinuity in the revenue function at $s=0$. Nonetheless, the seller can achieve as near to the first-best level of revenue as desired by setting $s=\varepsilon$.

This finding is easily understood in terms of a fundamental result for resource allocation problems under asymmetric information —that an economic agent earns rents reflecting the degree to which it holds (payoff relevant) *private* information. Here, the winning firm's bidding profit (or rent) is $\pi_i = r_1 - r_2 = s(v_1 - v_2)$. Making the firm's payment depend on v_i, assuming this is observable *ex post*, reduces the variation in private information (i.e., reservation prices) across bidders, and, therefore, reduces this rent. In the limit where $s=\varepsilon$, this bidding profit is driven to zero. As Paul Milgrom (1985) notes, the use of contingent pricing is one example of the "linkage" principle—linking price to a variable which is affiliated with bidders' private information reduces the winning bidder's rent. Dollar for dollar, this raises the seller's expected revenue since (1) can be rewritten

$$E[R]=E[v_1]-E[\pi_1]. \tag{1'}$$

In its simplest version, the lessons of Hansen's model for real-world applications are straightforward. In a corporate acquisition the target would prefer to retain a near 100 percent profit share while transferring management control to the most efficient acquirer. Similarly, the federal government would "sell" mineral and oil leases by awarding management contracts. Finally, in a competitive procurement, the buyer would award a cost-plus contract to the low-cost winning bidder.

I. Limitations

In principle, auctioning an *epsilon* share to the highest bidder achieves a first-best outcome, provided: (*i*) the value v_1 is observable *ex post*, (*ii*) this value is unaffected by *ex post* actions of the firm, and (*iii*) the auction procedure indeed selects the high value firm. In a more realistic model of the contracting setting, some or all of these provisos are likely to be violated, causing a significant reduction in the performance of (extreme) contingent price schemes. The present analysis first points out the welfare costs due to the possibility of *moral hazard* (items *i* and *ii*) before taking up my main topic, the costs stemming from *adverse selection*.

Under fixed price bidding, the seller's payoff is determined at the conclusion of the auction. By contrast, under contingent pricing, the seller enters into a contractual relationship with the winning firm. Thus, the seller and winning bidder face the standard set of principal-agent problems that (by now) are very well known. The revenue of the seller (the principal) will be reduced relative to the first-best optimum to the extent that 1) monitoring v_i (perfectly or imperfectly) is costly, and/or 2) the winning firm (the agent) is prone to *moral hazard*, that is, has the opportunity or the incentive to take actions *ex post* that reduce the value of v_1, so reducing the seller's revenue. Because the value v_1 typically incorporates revenues and costs that are more or less difficult for the principal to monitor, one type of moral hazard occurs when the firm misrepresents elements of v_1, such as exaggerating actual costs, and profits thereby. A second type of moral hazard stems from the firm's incentive to take inefficient actions. As is well known, under contingent price contracts, the agent firm will generally have insufficient incentives to undertake an efficient level of "effort." Thus, it may exhibit managerial slack or pay insufficient attention to cost control, passing on "unnecessary" costs to the seller thereby reaping (internal) benefits.

As is well known, the effects of imperfect monitoring and moral hazard are mitigated by increasing the agent's profit share—that is, reducing the degree of price contingency in the contract.[3] (Moral hazard problems are eliminated altogether by holding the agent to a fixed-price contract.) Thus, in the first instance, there is a tradeoff between the competitive benefit and the moral hazard cost of increasing the degree of contingent pricing in the auctioned contract.[4]

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[3] The most well-known analyses are in noncompetitive contexts where the tradeoff is between risk-sharing and moral hazard effects. A recent representative treatment is Martin Weitzman (1980).

II. Adverse Selection

I now make my main point: that increasing the degree of price contingency in the auction leads to adverse selection, that is, it reduces the likelihood that an efficient (high value) firm will be selected in the first place.[5] Intuitively, this is clear from the knife-edge nature of the *epsilon* profit share contract. Since firm reservation prices $r_i = \varepsilon v_i$ are nearly identical (and nearly zero), the bids, though they follow the same ordering as values, must be nearly identical also. But this means that any added factor (even pure white noise) can destroy the identity of the bid and value orderings and, therefore, lead to inefficient selection in the auction.

Inefficient selection is also a potential problem under Hansen's proposal for profit-share bidding. This is explicit in his model of stock bidding for a target firm. Here, potential acquirers differ with respect to their own market valuations, x_i, as well as their potential acquisition values v_i for the target. Hansen demonstrates the strong result that the target can use profit-share bids to identify correctly the high-value acquirer ($v_{(1)}$), *provided* it knows and can separate out the effects of the x_i. In the absence of such knowledge, however, profit share bidding will be inefficient—that is, the high-value acquirer need not be selected for the transaction.

To model the effects of adverse selection, let us suppose that in place of $r_i = sv_i$, firm reservation prices are given by

$$r_i = sv_i + z_i, \tag{2}$$

where z_i is an additive factor. Assume that each z_i is independent of v_j and z_k, for $j = 1, \ldots, n$ and $k \neq i$, and that each z_i comes from probability distribution $G(z)$ having a positive variance. In the English auction, each firm bids $b_i = r_i$ as before and the firm with the highest reservation price wins the auction. However, this firm need not hold the highest value since r_i depends on z_i as well as v_i. Indeed, as the firm's profit share shrinks, the bid ordering reflects less and less the underlying value ordering. In the limit $s = \varepsilon$, the bids b_i are ordered solely by the additive factors z_i and so are independent of values.

Returning to (1′), it is easy to trace the effect on seller revenue of changes in the firm's cost-sharing rate. A reduction in s has two countervailing effects: As before, it has the competitive benefit of reducing the winning firm's expected bidding profit (the second bracketed term) and so raises seller revenue. At the same time, lowering s leads to less efficient selection of high value firms, thus lowering revenue via the first bracketed term.

A good illustration of the inherent tradeoff between selection and competitive effects is furnished by the case of a competitive procurement. Here, numerous potential suppliers vie to win a production or service contract offered by a single buyer. Suppose that firm i's total cost of fulfilling the contract is the sum of a production cost component c_i and a required profit amount z_i. Firms differ with respect to c_i and z_i, and only the firm itself knows its characteristics *ex ante*. (I abstract from the possibility of moral hazard by assuming that a firm cannot influence its characteristics.) After contract award and completion, the buyer is able to observe the winning firm's cost c_1, but the firm's profit requirement z_1 remains unobservable. In response to this last fact, the buyer's procurement contract is written to be contingent on c_1 alone (and not on total cost $c_1 + z_1$). Under a linear contract, the buyer's payment to the winning firm is

$$P = b + (1 - s)c_1. \tag{3}$$

Here, the term b is the firm's fixed-profit fee. In turn, s is the share of observable production cost that the firm bears; the buyer's reimbursement is the amount $(1 - s)c_1$. At

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[4] For recent treatments (including formal models) of this tradeoff, see R. Preston McAfee and John McMillan (1984), R. Engelbrecht-Wiggans (1985), and Jean-Jacques Laffont and Jean Tirole (1985).

[5] John McCall (1970) presents a simple example of adverse selection in contracting.

$s=1$, the contract is fixed price, $P=b$. At $s=0$, it is cost-plus, $P=b+c_1$. The buyer is said to institute a linear "incentive" contract for $s \in (0,1)$.

The auction procedure is as follows. 1) The buyer sets s. 2) Firms submit profit fee bids b_i. 3) The firm submitting the lowest profit fee bid b_1 is chosen to undertake the contract at terms b_2 (the second-lowest bid) and s. Note that firm i's reservation price (its break-even fee) is $r_i = sc_i + z_i$, and firms bid according to $b_i = r_i$. The buyer's expected payment is

$$(4) \qquad E[P] = E[b_2] + (1-s)E[c_1]$$
$$= E[c_1 + z_1] + E[\pi_1],$$

after using the fact $\pi_1 = b_2 - r_1 = b_2 - sc_1 - z_1$. The buyer's payment must cover the winning firm's production cost and profit requirement plus its bidding profit. As noted earlier, there is a basic tradeoff between the "selection" and "competitive" effects of varying s. As s falls, the first term increases (since selection of the low total-cost firm becomes less and less likely) but the second term falls: $\partial E[c_1 + z_1]/\partial s < 0$ for $s \in [0,1)$ and $\partial E[\pi_1]/\partial s > 0$ for $s \in (0,1]$. One can also show that the former and latter effects diminish to zero as the contract becomes respectively fixed price or cost-plus—that is, $\partial E[c_1 + z_1]/\partial s = 0$ at $s=1$ and $\partial E[\pi_1]/\partial s = 0$ at $s=0$. Consequently, the optimal sharing rate s^* lies in the open interval $(0,1)$—that is, neither a cost-plus nor a fixed-price contract is optimal.

III. Example

Suppose each c_i is drawn independently from normal distribution $N(\mu_c, \sigma_c)$ and each z_i is drawn from $N(\mu_z, \sigma_z)$. Then, the reservation prices $r_i = sc_i + z_i$ across firms are also independently and normally distributed. To compute the buyer's expected payment, first write $E[c_1 + z_1]$ as $E_{r_1}[E[c_1 + z_1 | r_1]]$. Since r_i and c_i are bivariate normal, as are r_i and z_i, the conditional expectation is *linear* in r_1 and can be easily expressed in terms of the data of the problem. Furthermore, the expectation of the jth lowest (i.e., jth order statistic) of n independent draws from a normal distribution can be written $E[r_j] = \mu - k_j \sigma$, where k_j is a statistical constant that depends on n. Thus, it is straightforward for one to compute $E[r_1]$ and $E[\pi_1] = E[r_2] - E[r_1]$. After carrying out these computations, one obtains

$$(5) \quad E[P] = \big[\mu_c + \mu_z - k_1\sigma_r - k_1 s(1-s)\sigma_c^2/\sigma_r\big] + \big[(k_1 - k_2)\sigma_r\big],$$

where $\sigma_r = (s^2\sigma_c^2 + \sigma_z^2)^{1/2}$ is the standard deviation of r_i. It is easy to check that $\partial E[P]/\partial s$ is negative at $s=0$ and positive at $s=1$ so that the optimal sharing rate occurs in the interior of the interval $(0,1)$.

Table 1 lists the expected cost consequences under fixed-price, cost-plus, and optimal contracts. In all cases, the distribution means are normalized to zero, $\mu_c = \mu_z = 0$. Thus, $E[P]$ and $E[c_1 + z_1]$ should be interpreted as the buyer's reduction in cost (relative to a zero mean) under a given cost-sharing rate. In addition, the standard deviation of z_i is normalized to one. Two types of conditions are varied: (*i*) the number of competing firms ($n = 3$, 6, or 10) and (*ii*) the standard deviation of production cost across firms ($\sigma_c = 5$ or 10). Since a firm's profit requirement is a small fraction of its total cost, one would expect that the z_i would vary less across firms than would the c_i. Thus, we examined the cases $\sigma_c/\sigma_z = 5$ or 10. One can make the following observations from Table 1:

1) As noted earlier, $E[c_1 + z_1]$ and $E[\pi_1]$ are respectively increasing and decreasing with s, other things equal. For $n > 3$, the magnitude of the former effect is (considerably) larger than the latter. Thus, in the majority of cases, the potential benefit from superior selection dwarfs the potential gain from rent reduction. Accordingly, the fixed-price contract dominates the cost-plus (i.e., *epsilon*) contract.

2) Other things (including s) equal, $E[P]$ decreases as n and/or σ_c increase—that is, the buyer is better off selecting from a larger number of firms or when cost variations across firms increase.

TABLE 1—EXPECTED PROCUREMENT COSTS

Number of Firms		$E[P]$	=	$E[c_1+z_1]$	+	$E[\pi_1]$
$n=3$						
$\sigma_c=5$	$s=.00$	0		$-.85$		$+.85$
	$s^*=.27$	-2.49		-3.92		$+1.43$
	$s=1.00$	0		-4.33		$+4.33$
$\sigma_c=10$	$s=.00$	0		$-.85$		$+.85$
	$s^*=.18$	-6.09		-7.84		$+1.75$
	$s=1.00$	0		-8.54		$+8.54$
$n=6$						
$\sigma_c=5$	$s=.00$	$-.64$		-1.27		$+.63$
	$s^*=.34$	-4.87		-6.12		$+1.25$
	$s=1.00$	-3.26		-6.47		$+3.21$
$\sigma_c=10$	$s=.00$	$-.64$		-1.27		$+.63$
	$s^*=.24$	-10.57		-12.21		$+1.64$
	$s=1.00$	-6.43		-12.76		$+6.33$
$n=10$						
$\sigma_c=5$	$s=.00$	-1.00		-1.54		$+.54$
	$s^*=.38$	-6.37		-7.53		$+1.16$
	$s=1.00$	-5.10		-7.85		$+2.75$
$\sigma_c=10$	$s=.00$	-1.00		-1.54		$+.54$
	$s^*=.27$	-13.42		-14.98		$+1.56$
	$s=1.00$	-10.05		-15.48		$+5.43$

Note: $\sigma_z=1, \mu_z=\mu_c=0$. Thus, $E[P]$ and $E[c_1+z_1]$ should be interpreted as the buyer's reduction in cost (relative to a zero mean) under a given cost-sharing rate.

3) Most important, an appropriately designed incentive contract is far superior to either the fixed-price or cost-plus extremes. Optimal sharing rates fall in the 20–40 percent range. Thus, superior performance is achieved by making the firm share a relatively small portion of costs. Such contracts perform well in terms of selection (compare $E[c_1+z_1]$ at s^* and $s=1$). At the same time, optimal sharing rates are low enough to limit firm bidding profits (compare $E[\pi_1]$ at s^* and $s=0$).

4) As one would expect, the optimal sharing rate rises when the number of competing firms and/or the ratio σ_z/σ_c increases. Either effect increases the marginal gain from better firm selection, implying a higher sharing rate s. (Of course, $s=\varepsilon$ is optimal as σ_z/σ_c approaches zero.)

Before leaving the example, it is worth noting the effects of allowing correlation between c_i and z_i. The most likely case is that c_i and z_i are negatively correlated reflecting the fact that a low-cost producer for a particular procurement may very well enjoy cost advantages in its other alternatives and, therefore, demand a high profit rate. Given negative correlation, contracts with low firm-sharing rates perform poorly—they tend to select firms with low profit requirements but high total costs. Furthermore, with negative correlation, the profit-reducing effect of low sharing rates is lessened or even reversed. As can be checked analytically, the optimal sharing rate increases with the degree of negative correlation between c_i and z_i other things equal. An extreme illustration (correlation of -1) is provided in the case that $z_i = C-(1-\varepsilon)c_i$, where ε is in the positive neighborhood of zero. Here, the variation in reservation prices *declines* as s increases. Thus, firm profits are reduced by *raising* the sharing rate (the reverse of the usual effect). Note also that a firm's total cost declines as c_i falls (z_i rises). As one can easily confirm, this leads to an extreme case of adverse selection: for any sharing rate $s<1-\varepsilon$, the *high* total cost firm from the competing population is selected. Nonetheless, by setting a fixed-price contract, $s=1$, the buyer secures a first-best outcome (perfect selection and a zero bidding profit for the winning firm).

IV. Conclusion

The discussion and analysis has demonstrated that the competitive benefits of auctions having contingent payments (clearly identified by Hansen and more recently by others) must be weighed against the accompanying costs stemming from moral hazard and (especially) adverse selection. Appreciation of this tradeoff offers a strong justification for the use of optimal incentive contracts in actual practice.[6]

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[6] The buyer can improve upon an optimal incentive contract by allowing each competing firm to submit a bid consisting of a cost-sharing rate and fixed fee, and rewarding high sharing rates and low fees in selecting the contractor. Such a procedure allows firms to better "signal" its private characteristics c_i and z_i. (In particular, a firm's submission of a high sharing rate signals a low production cost c_i.) This same point also applies to Hansen's stock bidding model in which a pair of private characteristics must be identified. The optimal structure of such signaling contracts is discussed in my 1986 article and is a promising field of future research.

Auctions with Contingent Payments: Comment

By JACQUES CRÉMER*

In an interesting paper in this *Review* (1985), Robert Hansen has shown that an auction with contingent payments can yield higher payoffs to the seller than standard cash auctions. The aim of this note is to show that this result can be strengthened: By using auction forms which combine contingent payments and cash transfers, the seller can do "nearly" as well as under full information.

Consider Hansen's first example. There is one "target" firm and n potential "acquiring" firms. The value of a 100 percent share in any of the acquiring firms is X, which is publicly known. The value, V_i, of the target firm to the i acquiring firm is private information. Suppose that the target firm is sold by the following auction mechanism. The acquiring firms announce numbers $p_1, p_2, \ldots, p_n$ between 0 and 1. If firm i bids the highest p_j, $j = 1, \ldots, n$, it wins the auction. Then, the stockholders of the target own proportion p_i of the merged firms, and receive from the stockholders of the winner of the auction a payment of $p_i \cdot c$, where $c > -X$ is a sum which can be negative and which will be determined later.[1] Hansen treats the case $c = 0$.

Then, if the value to firm j of the target is V_j, it is a dominant strategy to bid up to $p(V_j)$ such that

$$p(V_j) = V_j/(X + V_j + c).$$

The firm with the highest valuation wins the auction, and the high bid will be

$$p^* = p(V_2) = V_2/(X + V_2 + c),$$

where V_2 is the second-highest valuation. Then, letting V_1 be the value of the highest valuation, π, the payoff to the stockholders of the target is

$$\pi = p^*(X + V_1) + p^*c.$$

If c is chosen close to $-X$, p^* will be close to 1 and π close to V_1. The target firm has done nearly as well as it would have with full information. The stockholders of the target firm will actually own nearly all the merged firm, and the stockholders of the acquiring firm will be paid off. The target firm has found a mechanism to buy the acquiring firm which will add most to its value.

The same type of strategy, with the same results, can be followed for Hansen's other examples. When all the acquiring firms do not have the same value X, use Hansen's rule except that $Z(p_i) = p_i(X_i + c_i)/(1 - p_i)$ and that the winning firm, not only leaves proportion p_i of the conglomerate to the stockholders of the target, but also pays $p_i \cdot c_i$. When c_i converges to $-X_i$ for all i, the same result as above holds.

In the case of the oil lease auction, the seller acquires proportion q of the losses as well as the profits, and pays the winner βq. As β becomes very small, the seller's profit converges to $pV - c_1$, the greatest surplus he could accumulate under perfect information.

All this reinforces Hansen's main point: contingent payments can be a powerful method for improving auctioners' profits.

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[1] Note that in the limiting case where c is exactly equal to $-X$, the payoff to the bidder if it wins the auction with a share p is pV_1, hence it is in its best interest to announce $p = 1$. Actually, this is also the case for the other bidders, and the seller has no way of determining the most advantageous bid. This is the reason why we must limit ourselves to the cases where c is very close to, but different from $-X$ (a similar phenomenon is pointed out in the first paragraph of Section II of Hansen's paper).

The Conformity of Wage Indexation Models with the "Stylized Facts": A Comment

By ROGER KAUFMAN AND GEOFFREY WOGLOM*

In a recent paper, Gary Fethke (1985) uses data from five industrialized countries to show a consistent pattern of negative correlations between output and price innovations. He demonstrates why this pattern of correlations is consistent with optimal wage indexation, and then uses his empirical results to infer what the degree of wage indexation in each country must be for wage indexation to be optimal. Fethke estimates that wage indexation is high in four of his five countries; in the United States, wage indexation is estimated to be near zero.

A simple extension of Fethke's model allows us to estimate the degree of wage indexation unconditionally, and then independently test whether wage indexation is optimal. Using quarterly data similar to Fethke's, we reject the hypothesis of optimally indexed wages. Our unconditional estimates of the degree of wage indexation are uniformly low and insignificant. We argue that this result should not be surprising. Given the institutional pattern of wage setting, we believe it is more appropriate to test models of optimal indexation with annual data.

I. The Model

In Fethke's model, the output innovation is a positive function of the price innovation. This result is derived from an aggregate supply schedule with nominal wages that are less than perfectly indexed. The implicit aggregate supply schedule can be written as

$$(1) \qquad y = -c(w-p) + (1+c)\varepsilon,$$

where y, w, and p are the logs of the real output innovation, the wage innovation, and the price innovation, respectively; ε is a productivity innovation; c is a parameter from the production function. A more general form of Fethke's implicit wage equation can be written as

$$(2) \qquad w = \gamma p + x,$$

where γ is the degree of nominal wage indexation; x is an autonomous wage innovation, assumed to be white noise and uncorrelated with the other disturbances. Fethke implicitly assumes that there are no autonomous wage innovations. With this assumption, (2) can be substituted into (1) to derive Fethke's specific aggregate supply equation, his equation (1). Notice, however, that without the x term, the model would predict perfectly correlated wage-and-price innovations.

In work done with Richard Jackman (1984) and Andrew Policano (1981), Fethke has shown that optimal wage indexation (or one kind of optimal monetary rule) implies that the regression coefficient of the output innovation on the price innovation, r_{yp} will equal the regression coefficient of the productivity disturbance on the price innovation $r_{\varepsilon p}$. With this result, he derives the restriction he uses to estimate γ. His restriction can be derived in our elaboration of his model by regressing both sides of (1) on the price innovation to yield

$$(3) \qquad r_{yp} = -c(r_{wp} - 1) + (1+c)r_{\varepsilon p}.$$

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TABLE 1—CONDITIONAL AND UNCONDITIONAL ESTIMATES OF WAGE INDEXATION

	1955: I–1981: IV		1961: IV–1981: IV			1961: IV–1985: IV		
	r_{yp}[a]	γ[a]	r_{yp}	r_{wp}	$(r_{wp}-r_{yp})$	r_{yp}	r_{wp}	$(r_{wp}-r_{yp})$
Canada	−.234	.766	−.348	.350	.698	−.558	.338	.896[b]
			(.330)	(.230)	(.388)	(.388)	(.201)	(.385)
Germany	−.175	.825	−.730[b]	.024	.754[b]	−.861[b]	.106	.968[b]
			(.276)	(.265)	(.365)	(.285)	(.257)	(.355)
Japan	−.037	.963	−.111	.203	.314[c]	−.055	.168	.223[c]
			(.123)	(.143)	(.191)	(.119)	(.128)	(.179)
U.K.	−.152	.848	−.309	.067	.376	−.269	.151	.419[c]
			(.207)	(.199)	(.324)	(.179)	(.170)	(.275)
U.S.	−1.051	−.051	−.515	.031	.545	−.161	.101	.262[c]
			(.390)	(.111)	(.418)	(.341)	(.095)	(.357)

Source: Gary Fethke (1985).

Note: Standard errors, which are not reported by Fethke, are in parentheses. All regressions in the U.K. start in 1963: IV. Our vector autoregressions are second order on the first differences of the logs and a constant. Wages are IMF series #65ey, hourly earnings in manufacturing; output data are IMF series #66ey, manufacturing production; prices are the OECD series on the CPI, and money supply is the OECD series on M1.

[a] Fethke is the source of the data that appear in cols. 1 and 2 only.

[b] Significantly different from zero at the 5 percent level.

[c] Significantly different from one at the 5 percent level.

Using the restriction of optimal wage indexation in (3), and rearranging yields Fethke's restriction

$$r_{wp}-r_{yp}=1. \tag{4}$$

In Fethke's model $\gamma=r_{wp}$, so he uses (4) to estimate γ from his estimates of r_{yp}. In our model, r_{wp} is a positively biased estimate of γ. (From (2), we can infer that $r_{wp}=\gamma+r_{xp}$, and one can show that $r_{xp}>0$.) Nevertheless, (4) still holds if wages are optimally indexed.

II. Empirical Results

Our tests are based on vector autoregressions for Fethke's five countries with a similar, but not identical data set. For our tests we expanded Fethke's model to a four-variable model that includes the nominal wage, and then regressed the output and wage residuals on the price residual to estimate r_{yp} and r_{wp}. Fethke's and our results are reported in Table 1. Fethke's estimates of r_{yp} are given in the first column of numbers; the second column lists his conditional estimates of γ (that is, col. 2 = 1 + col. 1). Columns 3, 4, 6, and 7 give our unconditional estimates of r_{yp} and r_{wp} for two sample periods.

The first item to note in Table 1 is that only for Germany is our estimate of r_{yp} significantly different from zero. On the other hand, the fact that all our estimates are negative does support the hypothesis of optimal indexation. The estimates are also generally comparable to Fethke's. Next, however, note the very low estimates of r_{wp}, particularly relative to Fethke's conditional estimates. In no case are we able to reject the hypothesis that r_{wp} is zero.

The results of our test of the restriction $r_{wp}-r_{yp}=1$ appear in columns 5 and 8. In the shorter sample period, only Germany provided results that were significantly different from zero and insignificantly different from one. The results for Japan were significantly different from one and thus rejected the hypothesis of optimal indexation. For the other three countries we could neither reject optimal indexation nor zero indexation. For the longer sample period, the results for both Canada and Germany were consistent with optimal indexation, but the results for the other three countries rejected optimal indexation.

III. Conclusion

We believe that our negative results are due to the use of an inappropriate time interval for testing for indexation. Wages in both Europe and North America are typically fixed for annual periods. Even the U.S. union contracts that call for explicit quarterly cost-of-living adjustments do not index wages on the basis of the *current* CPI. As reported in Kaufman and Woglom (1986), cost-of-living clauses in union contracts adjust the current wage to changes in the value of the CPI lagged at least one month, and in some cases lagged as much as six months. Thus we believe that a quarter is too short a time period to expect wage indexation to play an important role. In another paper (Kaufman and Woglom, 1987), we estimate a similar model with annual data and find that for 19 OECD countries, 15 of the estimates of r_{wp} are reasonable and significantly different from zero. We also find evidence that the cross-country differences in wage indexation are due, at least in part, to differences in the optimal degree of wage indexation.

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Spatial Competition and Vertical Integration; Cement and Concrete Revisited: Comment

By RONALD N. JOHNSON AND ALLEN M. PARKMAN*

In this *Review* (1983), Mark McBride reconsiders the Federal Trade Commission's (FTC) enforcement policy toward vertical mergers between cement and ready-mix concrete firms. In response to a significant increase in acquisitions of ready-mix concrete firms by cement manufacturers during the 1960's, the FTC undertook a series of legal actions to block or dissolve the mergers. The actions of the FTC constituted one of the most intensive efforts undertaken to date to challenge vertical mergers in a single industry.[1] McBride (p. 1012) notes that the actions of the FTC provoked considerable debate concerning the motivation for the mergers both in the industry and in academe. A significant number of articles were published advancing various reasons for the mergers. In addition to the FTC's main contention that the mergers were motivated by a desire for captive markets, it has been suggested that there were economies of integration or that the mergers were the outcome of an erroneous view of the potential benefits to foreclosure held by executives in the beleaguered cement industry.[2] McBride's 1983 paper offers another explanation for the mergers. His argument is that vertical integration was undertaken to avoid rigid oligopolistic pricing in the cement industry.[3] The empirical results presented by McBride suggest that vertical integration was a significant factor in the decline of cement prices in the 1960's.

The purpose of this comment is to point out some of the problems with McBride's analysis. In particular, we show that the experimental design of his testing equation is faulty and does not offer a test of his hypothesis. As a result, McBride's analysis does not provide convincing evidence on whether cement firms vertically integrated to avoid rigid oligopolistic pricing, or if cement firms were merely reacting to prices that had already begun to decline. Our intent, however, is not to challenge McBride's contention that vertical integration can provide lower prices to consumers. Rather, we would argue that the evidence presented at the FTC hearings involving cement and ready-mix concrete firms as well as McBride's and others' analyses illustrate the problems in discerning the motives for mergers.[4]

I. Vertical Integration and Cement Prices

McBride's 1983 hypothesis is that integrated cement firms are in a position to make less visible and less readily matched reductions in the oligopolistically determined cement prices by cutting the price of ready-mix concrete. He also argues that integrated firms may "accept lower profits at the concrete level to avoid underutilization at the cement level" (p. 1017). However, in both cases, rivals will eventually detect and react to lost market shares, although the reaction is assumed to be slower than to direct cuts in the price of cement. The end result is that vertical integration weakens oligopolistic pricing discipline. Integration is

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[1] See the *Economic Report*... of the FTC (1966).

[2] See, for example, Merton Peck and John McGowan (1967) and Bruce Allen (1971).

[3] Wesley Liebeler also argued that, "the most plausible explanation of mergers between cement and ready-mixed concrete firms seems to be a desire on the part of individual cement firms to increase their output by avoiding the oligopolistic price structure of the cement industry" (1968, p. 1201).

[4] Discerning the motives for a merger is of increasing importance as the Merger Guidelines now invite an economies defense. For a discussion of this policy change, see Oliver Williamson (1986).

TABLE 1—VERTICAL INTEGRATION AND CEMENT PRICES

Variable	Regressions 1	2	3	4
$UTIL_{i,t}$	−.244	−.320	−.562	−.711
	(−2.35)	(−1.66)	(−3.08)	(−4.06)
$UTIL_{i,t-1}$	.175	.418	.252	.164
	(1.51)	(2.22)	(1.44)	(0.96)
$UTIL_{i,t-2}$	.260	.366	−.048	−0.96
	(2.64)	(2.35)	(−.29)	(−.63)
$PLANTS_{i,t}$	−.038	−.004	−.009	−.019
	(−2.25)	(−.21)	(−.50)	(−1.03)
$DINTG_{i,t}$	.021	.026	.013	.038
	(1.30)	(1.46)	(.79)	(1.36)
$INTG_{i,t}$	−.026	−.045	−.011	−.074
	(−3.71)	(−5.03)	(−1.07)	(−8.17)
Intercept	3.52	3.11	9.60	4.08
	(15.98)	(12.52)	(7.47)	(16.59)
Time Trend			−.03	
			(−5.13)	
$\bar{R}^2$	.575	.504	.585	.608

Note: The t-statistics are shown in parentheses. The regressions included dummy variables for region of observation. Data on *UTIL*, *PLANTS*, and cement prices are from U.S. Bureau of Mines, *Minerals Yearbook*. Cement prices were deflated using the Producer's Price Index.

expected to have a negative effect on concrete prices and a lagged negative effect on cement prices (see McBride, p. 1018).

To test the above implications, McBride looks at yearly average cement prices net of transportation costs in seventeen different U.S. Bureau of Mines regions for the period 1958 through 1966. He does not attempt to look at concrete prices directly, but rather infers that concrete prices lead cement prices. The cross-sectioned time-series testing equation used by McBride contains a set of dummy variables to account for regional variations and a set of explanatory variables. Included in the testing equation were current and lagged measures of capacity utilization in region i and time period t, $UTIL_{i,t}$, the number of cement plants, $PLANTS_{i,t}$, and two variables, $DINTG_{i,t}$ and $INTG_{i,t}$, that measure the number of vertical mergers in region i, year t, and the cumulative number of acquisitions in region i prior to period t, respectively. The capacity utilization rate variables were included to test for price flexibility. According to McBride, if prices were highly flexible, as in the idealized competitive model, there should be no correlation between cement prices and lagged capacity utilization.[5] The $PLANTS_{i,t}$ variable is McBride's proxy for the degree of competition. The variables $DINTG_{i,t}$ and $INTG_{i,t}$ are McBride's key variables. If "the estimated coefficients are negative, vertical integration is undermining oligopolistic price discipline and bringing prices down" (McBride, p. 1019).

Table 1 contains four regression runs using the variables defined above. The first regression results are those that appeared in McBride's 1983 article. The estimated coefficient on the variable *DINTG* is not significantly different from zero at the 5 percent level, but the coefficient on *INTG* is negative and significant. McBride's interpretation is that vertical integration did reduce prices but the process operated with a lag. Regression 2 is our attempt to reproduce McBride's estimates. Qualitatively, the results are very similar, but they do differ. One possible explanation is that our tabulations of vertical mergers or assignment to region differs from that used by McBride.[6] However, there is no need to quibble about an exact count of the mergers as a more fundamental estimation problem exists.

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[5] The difficulties inherent in relating rigid prices to policy implications are discussed by Dennis Carlton (1986).

The basic problem with McBride's approach emanates from the fact that between 1958 and 1966 cement prices, in real terms, fell in each and every region. Thus, the dependent variable exhibits a downward trend while the prime explanatory variable, *INTG*, is by construction a nondecreasing variable. When a time trend variable was included (Regression 3) as a means for detrending the variables, the coefficient on *INTG* was no longer significant. While that suggests McBride's results may be spurious, the inclusion of a deterministic time trend may also suppress a variable that is in fact significant. However, the results shown by Regression 4 make it clear that McBride's experimental design is faulty as it is not capable of distinguishing fact from fiction. For Regression 4 we reconstructed the variable *DINTG* using randomly drawn numbers and then recomputed the variable *INTG*.[7] The results, Regression 4, show that the signs on the coefficients of the variables *DINTG* and *INTG* are similar to that reported by McBride. In particular, the coefficient on the variable *INTG* remains negative and significant. Given that numerous runs using random numbers produced a similar significant result, it follows that the specification used by McBride is not capable of testing the hypothesis that vertical integration lowers cement prices. Furthermore, the general decline in cement prices commenced prior to most of the acquisitions and some of the regions experienced no vertical integration activity. Thus, it is unlikely that some other specification would reveal a convincing negative lag effect of vertical acquisition activity on cement prices.[8]

As a final point, we note that it is essential to McBride's argument that prior to the vertical acquisitions cement prices be determined in an oligopolistic environment. But, attempts to portray many of the markets in which vertical integration occurred as highly oligopolistic will have to deal with evidence to the contrary. For example, in 1964 there were eighteen cement manufacturers selling in the New York metropolitan area, an area that experienced more vertical merger activity than any other.[9] The top four firms accounted for 53.4 percent of total shipments in the New York market and the top eight had 70.5 percent of the market.[10] Also important is the general record of aggressive price competition and the entry of new firms in other regions.[11] In its final order in the *Marquette* case, the FTC argued that, "What evidence there is suggests that this merger was intended to secure an outlet for Marquette's cement and was in fact a response to increased competition and declining prices in the cement market."[12]

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[6] For the regressions shown in Table 1 we used McBride's tabulation of mergers (1979, Appendix A).

[7] For the results shown in Table 1, Regression 4, the variable *DINTG* was generated by assuming that the probability of a merger occurring in each year and in each region was .5. We also tried different probabilities and allowed for the possibility of two mergers in a given year. The results were similar to that reported in Table 1.

[8] We, like McBride, merely counted the mergers. The acquisitions, however, varied in both size and circumstances. Mergers such as that of Stewart Sand and Material Co. in Kansas City by Mississippi River Fuel Corp. illustrates another type of problem. Stewart was acquired in October of 1963; however, Mississippi River Fuel's only cement plant did not come on line until July of 1965. See *In the Matter of Mississippi River Fuel Corporation*, 75 F.T.C. 813 (1969) at 849. An additional problem has to do with McBride's use of regional prices. The implication of using those prices is that a vertical merger in a particular city is expected to have a significant impact on the regional price. Granted, a spatial competition model that implies a transmission of local price changes to other markets underlies McBride's analysis, but this does appear to be asking a lot, as only about 6 percent of all grey portland cement shipments in the United States were made to ready-mixed concrete firms owned by cement firms. See the *Economic Report*... of the FTC (p. 13).

[9] *In the Matter of Marquette Cement Manufacturing Company*, 75 FTC 32 (1969) at 48.

[10] *In the Matter of Marquette*..., at 51.

[11] See, for example, *In the Matter of Ash Grove Cement Company*, 85 FTC 1123 (1975) and *In the Matter of Texas Industries, Inc.*, 68 FTC 992 (1965) for a discussion of mergers in Kansas City and Memphis.

[12] *In the Matter of Marquette*..., at 104.

II. Conclusions

In this comment we have been critical of McBride's empirical analysis, arguing that his experimental design is not capable of testing the hypothesis that vertical integration reduced cement prices. We would also argue that given the limitations of the data, it may be difficult to come up with any convincing test. On more general grounds, however, McBride's contention that the vertical mergers in the cement industry were undertaken to avoid rigid oligopolistic pricing is troublesome. The record makes it difficult to conclude that many of the cement markets involved were of a tight oligopolistic nature. Our readings of the cases also suggest that speculation as to the motive for the mergers is likely to be endless as there are enough bits and pieces around to keep most of the hypotheses alive. The extensive amount of research done on the cement/ready-mix cases and the conflicting results shows how difficult it is to discern the motives for a merger. What is absent, however, is sufficient evidence that would demonstrate that the vertical mergers in the cement industry have or would create a significant probability of a harmful effect. Apparently, the FTC now concurs as the agency recently rescinded its enforcement policy regarding vertical mergers between cement and ready-mix concrete firms.[13]

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[13] 50 Federal Register 21507 (1985).

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Spatial Competition and Vertical Integration; Cement and Concrete Revisited: Reply

By MARK E. MCBRIDE*

Ronald Johnson and Allen Parkman claim in their comment that real cement prices fell in each and every region over the time period 1958 to 1966. This statement misleads the reader since prices did not decline monotonically or uniformly from 1958 to 1967. The magnitude of the decline depends on the choice of price deflators and the region of the country. Johnson and Parkman's replication uses a different deflator than my 1979 or 1983 studies. The choice of deflator does not alter any qualitative conclusions. Real prices increased from 1958 to 1962 or 1963. Real prices declined from 1963 or 1964 to 1966. In 1966 and 1967 real prices increased in some regions and declined in others. The change in real prices over the 1960–67 period varied, ranging from a slightly positive (<1%) to a decline of approximately 25 percent.[1] The decline in real prices occurred after a period of capacity expansion driven by technical change.[2]

In addition, Johnson and Parkman claim that "Furthermore, the general decline in cement prices commenced prior to most of the acquisitions and some of the regions experienced no vertical integration activity" (p. 752). Their statement provides an erroneous view of the time path of integration. Approximately 45 percent of the integration occurred prior to the general decline in prices after 1963 and approximately 51 percent occurred concurrently with the general decline in prices from 1964 to 1966.[3] This time path evidence neither confirms nor refutes my hypothesis, but instead, simply illustrates that real prices change in response to many influences.

The results of regressing the real price on a time trend and cumulative integration ($INTG_t$) for each of the eleven regions (out of seventeen) in which new mergers occurred yields negative signs for $INTG_t$ in eight of the regressions, but significant in only two. Further, the results of separate cross-section regressions, which are not subject to Johnson and Parkman's basic criticism, yield insignificant negative signs in six of the eight cross sections. Both formulations contain strong multicollinearities between time patterns, cumulative integrations, lagged variables, and the intercept. These collinearities are difficult to deal with given the low degrees of freedom; each time-series regression had 8 observations and each cross-section regression had 17 observations. First differencing the pooled time-series, cross-section model results yields an insignificant negative sign both for current and lagged changes in integration.

My dissertation reports the results of a full covariance model which includes dummies for time and cross section. The time dummies, in effect, provide a more flexible fit to the time path of the dependent variable by not imposing linearity. The inclusion of the time dummies drives the coefficient on cumulative integration positive and insignificant. I presented the results of principal component regression, an exploratory technique, which shows that by accepting a small bias to reduce variance the estimated coefficient for cumulative integration is significantly negative. Ridge regression, another exploratory technique, provides similar results when a trend variable or time dummies are included.

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[1] U.S. Bureau of Mines (1958–67) and *Economic Report of the President* (1987). Inclusion of lagged variables restricted regression to using information from 1960 to 1967. Further elaboration of these and other points can be obtained upon request from the author. An Appendix of additional evidence is also available upon request.

[2] U.S. Bureau of Mines (1958–67), and M. J. Peck and J. J. McGowan (1967).

[3] Forty-seven mergers occurred in the 17 regions used in the sample, with 6 of the regions experiencing no new integration. See my dissertation (1979) for a complete list of the mergers.

Johnson and Parkman contend that my theory requires prices to be determined in tightly oligopolistic markets and that SMSA level data contradicts the premise. They misinterpret or misunderstand both the theory and the nature of the regional markets. First, the theoretical model says that increasing the extent of overlap competition (for example, SMSA competition) among firms increases the probability that a firm will undertake nonprice competition (of which vertical integration is just one choice). This implies vertical integration is more likely in densely interrelated markets. Second, even though mergers affected only 6 percent of national sales, the percentages involved in a particular merger will substantially effect the unit costs and regional realized price of the involved cement firm given the very large economies of scale in cement and the extremely high fixed cost, low marginal cost nature of cement production in the short run.[4] Third, insufficient market share data on a sufficient large sample prevented any measure of mergers other than a simple count. Finally, any attempt to portray the cement industry as competitive must deal with strong evidence to the contrary. While the number of sellers is higher in a few selected overlap areas (for example, New York, Los Angeles), concentration in the regional markets is higher than Johnson and Parkman's portrayal. In 34 of 51 state areas, the top four firms accounted for over 75 percent of cement shipments while in only 3 areas did the top four account for less than 50 percent.[5] While states are unlikely to be the ideally defined market areas (my earlier work, 1979, 1983, used broader multistate groupings), neither does one SMSA constitute the relevant market for a single plant or firm. Over 90 percent of cement shipments moved less than 160 miles and 70 percent of shipments were intrastate shipments.[6] Concentration is high in the cement industry. The record of aggressive price competition occurred in selected contested overlap areas.

Johnson and Parkman base their criticism of my empirical work upon a misconceived view of the theory I developed and the data used to test the main hypothesis. The theory and empirical test develops a model of a firm selling in regional space with integration occurring in the areas of competitive overlap. The time trend of prices is nonlinear with increases in real prices early in the sample and nonuniform declines late in the sample. Vertical integration occurred prior to and concurrently with declining prices; not only after the general decline in prices commenced. While cited earlier empirical work does not provide irrefutable support that vertical integration lowers prices, the clear absence of a positive effect indicates misguided FTC merger policy as many authors, including myself, have argued. Finally, I agree with Johnson and Parkman that motives for mergers are difficult to discern. However, I do believe that extensive investigations of available data (however imperfect) can lead to conclusions useful for policy, as, apparently, has the FTC given its recently revised policies toward cement-concrete mergers.[7]

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[4] See F. M. Scherer (1980; pp. 91–98, Table T4.2).

[5] FTC *Economic Report* (1966, p. 7).

[6] FTC *Economic Report* (p. 18).

[7] 50 Federal Register 21507 (1985).

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Preliminary Announcement of the Program

ANNUAL MEETING
THE AMERICAN ECONOMIC ASSOCIATION

Chicago, Illinois, December 27–30, 1987

Sunday, December 27, 1987

10:00 A.M. EXECUTIVE COMMITTEE MEETING

Monday, December 28, 1987

8:00 A.M. DO MODELS PRODUCE CONSISTENT RESULTS? POLICY IMPACTS IN ECONOMETRIC MODELS: COMPARATIVE STUDIES
Presiding: PETER PAULY, University of Pennsylvania
Papers: LAWRENCE R. KLEIN, University of Pennsylvania
A New Round of Model Comparisons for the United States
BERT G. HICKMAN, Stanford University
Structure and Policy Response in Multicountry Models
F. GERARD ADAMS, University of Pennsylvania
Model Performance Comparisons in Models of NICs and LDCs
Discussants: ROGER BRINNER, DRI
STEPHEN MCNEES, Federal Reserve Bank of Boston
PAUL ARMINGTON, World Bank

8:00 A.M. MERGERS, EFFICIENT MARKETS, AND ECONOMIC PERFORMANCE
Presiding: ROBERT W. CRANDALL, The Brookings Institution
Papers: B. ESPEN ECKBO, University of British Columbia
The Profitability of Takeovers: Methodological Issues and Empirical Evidence
ELLEN MAGENHEIM AND F. M. SCHERER, Swarthmore College
Assessing the Long-Run Performance of Mergers and Takeovers
RICHARD B. DU BOFF, Bryn Mawr College, AND EDWARD S. HERMAN, University of Pennsylvania
The Promotional-Financial Dynamic of Merger Movements: An Historical Perspective
Discussants: DENNIS C. MUELLER, University of Maryland
RICHARD S. RUBACK, Harvard University

8:00 A.M. THE POLITICAL ECONOMY OF DEBT AND DEFICITS
Presiding: WILLIAM NISKANEN, Cato Institute
Papers: ALBERTO ALESINA, Carnegie-Mellon University, AND GUIDO TABELLINI, University of California-Los Angeles
The Political Economy of the Government Budget
KENNETH S. ROGOFF, University of Wisconsin-Madison
Sovereign Debt: When is to Forgive to Forget?
ALEX CUKIERMAN, Tel Aviv University, AND ALLAN MELTZER, Carnegie-Mellon University
A Political Theory of Government Debt
Discussants: ROBERT J. BARRO, Harvard University
HERSCHEL GROSSMAN, Brown University
JOHN FEREJOHN, Stanford University

8:00 A.M. INTERNATIONAL TRADE WITH IMPERFECT COMPETITION
Presiding: WOLFGANG MAYER, University of Cincinnati
Papers: JEFFREY BALDANI, Colgate University
Tariffs and Limit Pricing
MONTAGUE J. LORD, Inter-American Development Bank
Protectionism and Subsidies in International Commodity Trade
LYNNE PEPALL, Concordia University
A Clustering Analysis of the Effects of Import Penetration on Product Variety
RAFAEL ELDOR, Boston University and Tel Aviv University, AND DAN LEVIN, University of Houston
Trade Liberalization and Imperfect Competition: A Welfare Analysis
Discussants: JAMES LEVINSOHN, University of Michigan
HOWARD GRUENSPECHT, Carnegie-Mellon University

8:00 A.M. INFLATION AND FULL EMPLOYMENT
Presiding: ALAN S. BLINDER, Princeton University
Papers: JAMES TOBIN, Yale University
Unemployment and Inflation: The Tradeoff Revisited
ROBERT E. HALL, Stanford University
Full-Employment Unemployment: Why Has It Risen So Much?
ROBERT J. GORDON, Northwestern University
Wage Changes in the 1980s: Too Slow, and If So Why?
Discussants: OLIVIER J. BLANCHARD, Massachusetts Institute of Technology
JAMES MEDOFF, Harvard University
ROBERT TOPEL, University of Chicago

8:00 A.M. MONEY, CREDIT AND BANKS
Presiding: STUART GREENBAUM, Northwestern University
Papers: LAWRENCE M. AUSUBEL, Northwestern University
On Sticky Prices in the Credit Card Market
HERBERT BAER AND CHRISTINE PAVEL, Federal Reserve Bank of Chicago
The Impact of Banking Regulation on Financial Innovation
ROBERT S. CHIRINKO, University of Chicago, AND STEPHEN R. KING, Federal Reserve Bank of New York
Bank Loans, Information Accumulation, and the Monetary Transmission Process
SCOTT SUMNER, Bentley College
Futures Instrument Prices as Monetary Policy Targets
Discussants: CHARLES M. KAHN, University of Chicago
PAUL EVANS, Ohio State University

8:00 A.M. REGULATION AND LONG-TERM CONTRACTS
Presiding: JOHN PANZAR, Northwestern University
Papers: MICHAEL A. TOMAN, Resources for the Future
Rate Designs and Other Regulatory Reforms in Local Natural Gas Markets
KEITH CROCKER, Pennsylvania State University, AND SCOTT MASTEN, University of Michigan
On the Origins of Price Redetermination Provisions in Long-Term Contractual Relationships
MARK A. ZUPAN, University of Southern California
Reneging by Cable Operators on Their Franchise Promises: Opportunism or Economic Necessity?
Discussants: DAVID BUTZ, University of California-Los Angeles
STEVE WIGGINS, Texas A&M University
VICTOR GOLDBERG, Northwestern University

8:00 A.M. RACE, ETHNICITY, AND INEQUALITY
Presiding: DAVID SWINTON, Clark College
Papers: ARTHUR M. DIAMOND, JR., University of Nebraska-Omaha
The Career Paths and Subfield Choices of Minority Members of the American Economic Association
WALTER S. MCMANUS, University of Florida
Labor Market Effects of Ethnic Enclaves: Hispanic Men in the United States
JONATHAN G. SANDY, University of San Diego
Public and Private School Choice: The Influence of Public School Quality, Race and Religion
MAHLON R. STRASZHEIM, University of Maryland
Housing Segregation and Employment Patterns and Black Employment Opportunities
Discussants: WILLIAM DARITY, JR., University of North Carolina
MARGARET SIMMS, Joint Center for Political Studies

8:00 A.M. DYNAMIC ASPECTS OF REAL ESTATE MARKETS (Joint Session with the American Real Estate and Urban Economic Association)
Presiding: JAN K. BRUECKNER, University of Illinois-Urbana
Papers: WILLIAM WHEATON, Massachusetts Institute of Technology
Search, Turnover, and Vacancy in Urban Housing Markets
JOHN QUIGLEY, University of California-Berkeley
Real Estate Financing and the Dynamics of Labor Mobility
ROBERT VANORDER, University of California-Los Angeles
Housing, Inflation, and Capital Accumulation
Discussants: JAN K. BRUECKNER, University of Illinois-Urbana
ALEX ANAS, Northwestern University
ROBERT HELSLEY, University of British Columbia

8:00 A.M. ANALYTICAL APPROACHES TO ENVIRONMENTAL AND RESOURCE PROBLEMS IN DEVELOPING COUNTRIES—REVISED (Joint Session with Association of Environmental and Resource Economists)
Presiding: VERNON W. RUTTAN, University of Minnesota
Papers: ROBERT REPETTO, World Resources Institute
Sorting Out Issues and Analytical Techniques for Environmental and Resource Analysis in Developing Countries
JEREMY J. WARFORD, World Bank
Issues, Institutions, and Incentives in Environmental Regulation and Enforcement in Developing Countries
CARLISLE F. RUNGE, University of Minnesota
Behavioral and Policy Approaches to Common Property Resources in Developing Countries
Discussants: DANIEL W. BROMLEY, University of Wisconsin-Madison
ELIZABETH L. DAVID, Wisconsin Department of Natural Resources
RICHARD B. NORGAARD, University of California-Berkeley

8:00 A.M. INSTITUTIONAL DESIGN AND INDUSTRIAL ORGANIZATION (Joint Session with the Economic Science Association)
Presiding: ELIZABETH HOFFMAN, University of Wyoming
Papers: CHARLES R. PLOTT, California Institute of Technology
Airport Use Rights
R. MARK ISAAC AND STANLEY REYNOLDS, University of Arizona
Innovation and Market Structure

8:00 A.M. CONSUMER IGNORANCE IN MARKETS WITH ASYMMETRICAL INFORMATION (Joint Session with the Health Economics Research Organization)
Presiding: DONALD E. YETT, University of Southern California
Papers: MARTIN GAYNOR, Center for Health Economics Research and National Bureau of Economic Research, AND SOLOMON POLACHEK, State University of New York-Binghamton
Measuring Ignorance in the Market for Physician Services
ROBIN ALLEN, University of Chicago and U.S. Department of Justice, AND PAUL GERTLER, Harvard University
Regulation and the Provision of Quality to Heterogenous Consumers: The Case of DRG Pricing of Hospital Services
DAVID DRANOVE, University of Chicago, AND MARK SATTERTHWAITE, Northwestern University
Markets for Products with Imperfectly Observed Attributes
Discussants: JODY SINDELAR, Yale University
ROGER FELDMAN, University of Minnesota
CHARLES PHELPS, University of Rochester

8:00 A.M. MAINTAINING EMPLOYMENT GROWTH IN AN ERA OF GLOBAL COMPETITION (Joint Session with the Industrial Relations Research Association)
Presiding: KENNETH MCLENNAN, Machinery and Allied Products Institute
Papers: DANIEL J. B. MITCHELL, University of California-Los Angeles, AND RICHARD S. BELOUS, The Conference Board
International Trade and Employment: Dynamic Labor Market Implications
WAYNE VROMAN, Urban Institute
Responding to International Competition Through Wage Flexibility
SUSAN N. HOUSEMAN, University of Maryland, AND KATHARINE G. ABRAHAM, Massachusetts Institute of Technology
Industrial Restructuring: How Quickly Can the Work Force Adjust?
Discussants: GEORGE EADS, General Motors Corporation
SHELDON FRIEDMAN, United Auto Workers

8:00 A.M. ECONOMIC THOUGHT IN THE CIVIL RIGHTS MOVEMENT (Joint Session with the National Economic Association)
Presiding: BERNADETTE CHACHERE, Hampton University
Papers: WILHELMINA LEIGH, Congressional Budget Office
Fair Housing: Economic Motivation in the Civil Rights Movement and Today
JULIANNE MALVEAUX, University of California-Berkeley
Evolution of the Economic Thought of Dr. Martin L. King, Jr.
Discussant: BERNADETTE CHACHERE, Hampton University

8:00 A.M. "STRATEGIC" PROTECTIONISM VS. FREE TRADE: THEORY AND POLICY (Joint Session with the North American Economics and Finance Association)
Presiding: RODRIGUE TREMBLAY, University of Montreal
Panel: RICHARD D. LIPSEY, Queen's University and C. D. Howe Institute
Free Trade Protectionism and the Small Country
RONALD J. WONNACOTT, University of Western Ontario
A Key Consideration in the Policy Debate
JAMES A. BRANDER, University of British Columbia
The Theory Behind Strategic Protectionism
JONATHAN EATON, University of Virginia
Trade and Industrial Policy
ROBERT W. STAIGER, Stanford University
Rules and Discretion in Trade Policy
HERBERT G. GRUBEL, Simon Fraser University
Negotiating Rules Rather Than Outcomes

8:00 A.M. DEREGULATION OR REREGULATION: ECONOMIC EFFICIENCY IN PUBLIC UTILITIES AND TRANSPORT (Joint Session with the Transportation and Public Utilities Group)
Presiding: PATRICK C. MANN, West Virginia University
Papers: DOUGLAS N. JONES, Ohio State University
Regulatory Concepts, Doctrines, and Propositions: Casualties and Survivors
WILLIAM G. SHEPHERD, University of Massachusetts
Standards for Effective Deregulation
MICHAEL K. EVANS, Evans Economics
Has Deregulation Helped or Harmed the Transportation Sector?
Discussants: MARTIN T. FARRIS, Arizona State University
BENJAMIN J. ALLEN, Iowa State University

8:00 A.M. CHILDREN'S POVERTY
Presiding: GIAN S. SAHOTA, Harvard University
Papers: TIMOTHY M. SMEEDING, Vanderbilt University
Comparative Poverty and Economic Status Among Children in Several Industrialized Countries
JOHN STRAUS, Yale University
Households, Communities and Preschool Children's Poverty and Nutrition Outcomes: Evidence from Rural Cote d'Ivoire (Ivory Coast)
GIAN S. SAHOTA, Harvard University, AND RICARDO AROSEMENA, Ministry of Planning, Republic of Panama
An Analysis of Children's Poverty in the Republic of Panama
RICHARD BURKHAUSER, Vanderbilt University, AND GREG DUNCAN, University of Michigan
Life Events, Public Policy and the Economic Vulnerability of Children and the Elderly
Discussants: KATHY ANDERSON, Vanderbilt University
JOHN AKIN, World Bank

8:00 A.M. THE VOLATILITY DEBATE (Joint Session with the American Finance Association)
Presiding: MICHAEL GIBBONS, Stanford University
Papers: JOHN CAMPBELL, Princeton University, AND ROBERT SHILLER, Yale University
Stock Prices, Earnings, and Dividend Expectations
JAMES POTERBA, Massachusetts Institute of Technology, AND LAWRENCE H. SUMMERS, Harvard University
Mean Reversion in Stock Returns: Evidence and Implications
KENNETH WEST, Princeton University
Bubbles and Fads in Asset Prices
Discussants: STEPHEN LE ROY, University of California-Santa Barbara
KENNETH FRENCH, University of Chicago
ALLEN KLEIDON, Stanford University

10:15 A.M. INTERNATIONAL ECONOMIC POLICY COORDINATION
Presiding: STEPHEN J. TURNOVSKY, University of Illinois-Urbana and National Bureau of Economic Research
Papers: MATTHEW CANZONERI AND DALE W. HENDERSON, Georgetown University
International Policy Conflict and the Choice of a Monetary Policy Instrument
PATRICK KEHOE, University of Minnesota
Coordination of Fiscal Policies in a World Economy

ALBERTO GIOVANNINI, Columbia University
International Conflict and Coordination in Monetary Policy: The Case of the European Monetary System
Discussants: KENNETH ROGOFF, University of Wisconsin-Madison
MICHAEL DEVEREUX, University of Toronto
PARTHA SEN, University of Illinois-Urbana

10:15 A.M. WHAT IF ANY GOOD CAN COME FROM THE LEGAL PRIVATE CARTELS OF OTHER COUNTRIES?
Presiding: RICHARD LEVIN, Yale University
Papers: DOUGLASS KLEIN, Union College
What Have the British Gained or Lost from the Exceptions They Have Allowed Under the Restrictive Trading Practices Act?
DAVID AUDRETSCH, Middlebury College and IIM-Wissenschaftzentrum, Berlin
The Costs and Benefits of West Germany's Legal Cartels
TOSHIAKI NAKAZAWA, Keio University, Japan, AND LEONARD W. WEISS, University of Wisconsin-Madison
The Effects of Japan's Legal Cartels
Discussants: HIDEKI YAMAWAKI, IIM-Wissenschaftzentrum, Berlin
F. M. SCHERER, Swarthmore College

10:15 A.M. THE NATURAL RATE THEORY RECONSIDERED
Presiding: MARTIN FELDSTEIN, Harvard University and National Bureau of Economic Research
Papers: ROBERT J. BARRO, Harvard University
Natural Rate Theories and Recent Unemployment in the United States and the United Kingdom
ASSAR LINDBECK, Institute of International Economic Studies, Stockholm, AND DENNIS J. SNOWER, Birkbeck College, London
Long-Term Unemployment Equilibrium and Aggregate Demand
GEORGE A. AKERLOF AND JANET L. YELLEN, University of California-Berkeley
Fairness and Unemployment
Discussants: STANLEY FISCHER, Massachusetts Institute of Technology
ROBERT E. LUCAS, JR., University of Chicago

10:15 A.M. ECONOMICS OF FLEXIBLE MANUFACTURING
Presiding: EDWIN MANSFIELD, University of Pennsylvania
Papers: ERNST BERNDT, Massachusetts Institute of Technology, AND MELVYN FUSS, University of Toronto
Measuring Capacity Utilization for Multiproduct Firms
M. ISHAQ NADIRI, New York University, AND ROBIN C. SICKLES, Rice University
The Structure of Production and Technical Change in a Multiproduct Firm
ROMESH DIWAN AND J. RANDOLPH NORSWORTHY, Rensselaer Polytechnic Institute
Toward an Economics of Flexible Manufacturing
Discussants: LAURENCE ROSENBERG, National Science Foundation
FRANK M. GOLLOP, Boston College
KENNETH MCLENNAN, Machinery & Allied Products Institute

10:15 A.M. THE ECONOMIC INDEPENDENCE OF WOMEN AND ITS CONSEQUENCES
Presiding: JANICE FANNING MADDEN, University of Pennsylvania
Papers: MICHAEL LEEDS, Temple University
Female Occupational Status and the Location Decisions of Two-Worker Households
MARIANNE FERBER, University of Illinois-Urbana, AND HELEN BERG, Oregon State University
Labor Force Participation of Women and Survival Rates
RANDY ALBELDA, Hobart and William Smith Colleges, JANE LAPIDUS, University of Massachusetts, AND ELAINE MCCRATE, University of Vermont
What Price Economic Independence: Understanding Women's Economic Status in the Post-WWI Period
CORDELIA REIMERS AND MARJORIE HONIG, Hunter College
Prospects for Economic Independence Among Older Women
Discussants: CLAUDIA GOLDIN, University of Pennsylvania
JANET KOHLHASE, University of Houston
BARBARA WOLFE, University of Wisconsin-Madison

10:15 A.M. TRANSACTION-EXTERNALITIES AND DEMAND SPILLOVERS
Presiding: ROBERT W. CLOWER, University of South Carolina
Papers: RUSSELL COOPER, University of Iowa, AND JOHN HALTIWANGER, Johns Hopkins University
Inventories, Demand Spillovers, and Persistence
ANDREI SHLEIFER AND ROBERT W. VISHNY, University of Chicago
The Efficiency of Investment with Aggregate Demand Externalities
PETER HOWITT AND R. PRESTON MCAFEE, University of Western Ontario
Endogenous Business Cycles with Transaction-Externalities
Discussants: NOBUHIRO KIYOTAKI, University of Wisconsin-Madison
STEVE DAVIS, University of Chicago

10:15 A.M. THE BUSINESS CYCLE: FACT OR ARTIFACT?
Presiding: BERT G. HICKMAN, Stanford University
Papers: FRANCIS X. DIEBOLD AND GLENN D. RUDEBUSCH, Board of Governers of the Federal Reserve System
Are Business Fluctuations Cyclical?
GEORGE W. EVANS, Stanford University
Persistence, Trend, and Cycle in Output and Unemployment
JAMES H. STOCK, Harvard University, and MARK W. WATSON, Northwestern University
What Do Leading Indicators Lead?
Discussants: JOHN Y. CAMPBELL, Princeton University
PETER K. CLARK, Stanford University
ROBERT J. SHILLER, Yale University

10:15 A.M. ISSUES IN ARTS POLICY
Presiding: DICK NETZER, New York University
Papers: MICHAEL O'HARE, Harvard University
The International Distribution of the Stock of Art
C. D. THROSBY, Macquarie University, Australia, AND GLENN A. WITHERS, La Trobe University, Australia
Individual and Collective Demand for the Arts
WILLIAM D. GRAMPP, University of Illinois-Chicago
The Rent in the Fabric
Discussants: J. MICHAEL MONTIAS, Yale University
GORDON TULLOCK, George Mason University
ROGER W. WEISS, University of Chicago

10:15 A.M. MICRO FOUNDATIONS OF MONEY AND CREDIT
Presiding: HERBERT GINTIS, University of Massachusetts
Papers: PERRY MEHRLING, Barnard College
A Credit Theory of Money
GARY DIMSKY, University of Southern California
A Theory of Financial Innovation
MICHAEL WOODFORD, University of Chicago
Inside Money and Economic Stability
Discussants: HYMAN MINSKY, Washington University
STEVEN MARGLIN, Harvard University
STEPHEN WILLIAMSON, Federal Reserve Bank of Minneapolis

10:15 A.M. TAX POLICY AND INVESTMENT: A RECONSIDERATION
Presiding: CHARLES R. HULTEN, University of Maryland
Papers: STEVEN FAZZARI, Washington University and Federal Reserve Bank of St. Louis, R. GLENN HUBBARD, Northwestern University and National Bureau of Economic Research, AND BRUCE C. PETERSON, Northwestern University and Federal Reserve Bank of Chicago
Tax Policy, Investment, and Financing Constraints
ROBERT S. CHIRINKO, University of Chicago and National Bureau of Economic Research
Tax Policy, Investment Spending, and the Lessons from the 1980s
JAMES R. HINES, Princeton University and National Bureau of Economic Research
Investment Tax Incentives and Frequent Tax Reforms
Discussants: ALLEN SINAI, Shearson Lehman Brothers/American Express, Inc.
CHARLES R. HULTEN, University of Maryland

10:15 A.M. STRATEGIES AND GOALS FOR FULL EMPLOYMENT
Presiding: NANCY S. BARRETT, American University
Papers: JAMES K. GALBRAITH, University of Texas-Austin
Expected Price Stabilization, the Composition of Employment, and Goals for Countercyclical Policy
MICHAEL MUSSA, Council of Economic Advisors
Real Wages, Productivity, and Employment
DONALD A. NICHOLS, University of Wisconsin-Madison
What Do We Know About the Barriers to Full Employment?
Discussants: ORLEY ASHENFELTER, Princeton University
ROBERT M. SOLOW, Massachusetts Institute of Technology
HOWARD WACHTEL, American University

10:15 A.M. DEREGULATION AND PRIVATIZATION: AN INTERNATIONAL PERSPECTIVE
Presiding: GLENN C. LOURY, Harvard University and U.S. Department of Education
Papers: RICHARD E. CAVES, Harvard University
The Case of Great Britain
WILLIAM JAMES ADAMS, University of Michigan
The Case of France
INGO VOGELSANG, Boston University
The Case of Germany
GARY R. SAXONHOUSE, University of Michigan
The Case of Japan
Discussants: WILLIAM G. SHEPHERD, University of Massachusetts
SHARON M. OSTER, Yale University

10:15 A.M. THE POLITICAL ECONOMY OF GRAMM-RUDMAN
Presiding: JOSEPH A. PECHMAN, The Brookings Institution
Papers: CHARLES L. SCHULTZE, The Brookings Institution
The Economics of Gramm-Rudman
WILLIAM NISKANEN, Cato Institute
The Political Economy of Gramm-Rudman
ALLEN SHICK, University of Maryland
Escapist Politics: Budgeting under Gramm-Rudman Constraints
Discussants: ROBERT E. HALL, Stanford University
RICHARD ZECKHAUSER, Harvard University
VAN DOORN OOMS, U.S. House Budget Committee

10:15 A.M. THE VALUE OF FULL EMPLOYMENT (Joint Session with the Association for Social Economics)
Presiding: WARREN J. SAMUELS, Michigan State University
Papers: ALFRED EICHNER, Rutgers University
The Artificial (Theoretical) and Real (Supply-Side) Obstacles to Full Employment
PHILIP A. KLEIN, Pennsylvania State University
What's "Natural" About the Unemployment Rate?
WALLACE PETERSON, University of Nebraska-Lincoln
Why Full Employment?
HOWARD J. SHERMAN, University of California-Riverside
What Actually Happens to Unemployment and Why
Discussants: WILLIAM DUGGER, DePaul University
STEPHEN WOODBURY, Michigan State University

10:15 A.M. COMPETITION IN HOSPITAL MARKETS (Joint Session with the Health Economics Research Organization)
Presiding: STUART GUTERMAN, Health Care Financing Administration
Papers: DEAN E. FARLEY AND CHRISTOPHER HOGAN, National Center for Health Services and Health Care Technology Assessment
Specialization in the Market for Hospital Services
S. GROSSKOPF, Southern Illinois University-Carbondale, AND V. VALDMANIS, Vanderbilt University
The Relative Performance of Public and Private, Not-for-Profit Hospitals: A Nonparametric Approach
JEAN M. MITCHELL, Johns Hopkins University, AND GARY M. FOURNIER, Florida State University
Does Competitive Entry Lead to Higher Costs and Excessive Services in the Hospital Industry?
Discussants: FRANK A. SLOAN, Vanderbilt University
WARREN GREENBERG, George Washington University
JERRY CROMWELL, Center for Health Economics Research

10:15 A.M. STUDIES IN MATHEMATICS AND ECONOMIC ANALYSIS (Joint Session with the History of Economics Society)
Presiding: INGRID RIMA, Temple University
Papers: PHILIP MIROWSKI, Tufts University
The How, the When, and the Why of Mathematical Technique in Economics
RANDALL BAUSOR, University of Massachusetts-Amherst
Lyapounov Techniques, Economic Dynamics, and Classical Thermodynamics: A Comparison
JUDY KLEIN, Mary Baldwin College
Early Applications of Geometric Form to Time, Money, and Choice
Discussants: ROGER MCCAIN, Fordham University
NANCY WULWICH, University of California-Fresno

10:15 A.M. OMICRON DELTA EPSILON BIENNIAL CONVENTION/THE JOHN R. COMMONS AWARD LECTURE (Joint Session with Omicron Delta Epsilon)
Presiding: G. RANDOLPH RICE, Louisiana State University
Speaker: (TO BE ANNOUNCED)

10:15 A.M. POPULATION GROWTH AND ECONOMIC DEVELOPMENT (Joint Session with the Society for Policy Modeling)
Presiding: DOMINICK SALVATORE, Fordham University
Papers: T. N. SRINIVASAN, Yale University
Population Growth and Development
DAVID E. BLOOM AND RICHARD B. FREEMAN, Harvard University
Population Growth and Labor Markets in Developing Countries
MARK R. ROSENZWEIG, University of Minnesota
Human Capital, Population Growth, and Economic Development
Discussants: DAVID A. LAM, University of Michigan
J. VERNON HENDERSON, Brown University
T. PAUL SCHULTZ, Yale University

12:30 P.M. AEA/AFA JOINT LUNCHEON
Presiding: STEPHEN ROSS, Yale University
Speaker: MERTON MILLER, University of Chicago

2:30 P.M. POLICY ASPECTS OF SMOKING AND ALCOHOL
Presiding: W. KIP VISCUSI, Northwestern University
Papers: WILLARD MANNING, EMMETT KEELER, JOSEPH NEWHOUSE, ELIZABETH SLOSS, AND JEFFREY WASSERMAN, Rand Corporation
Economic Costs of Poor Health Habits
THOMAS C. SHELLING, Harvard University
The Regulation of Smoking in Public Places
THOMAS G. POGUE AND LARRY G. SGONTZ, University of Iowa
Optimal Taxation of Alcoholic Beverages
Discussants: BERNARD S. FRIEDMAN, Northwestern University
MICHAEL MOORE, Duke University
GERARD RUSSO, University of Hawaii

2:30 P.M. APPLICATIONS OF INTERNATIONAL COMPARISONS OF PRICES AND QUANTITIES
Presiding: RUDIGER W. DORNBUSCH, Massachusetts Institute of Technology
Papers: ROBERT SUMMERS AND ALAN HESTON, University of Pennsylvania
What Has Been Learned About Prices and Quantities From International Comparisons: 1987
IRVING B. KRAVIS, University of Pennsylvania, AND ROBERT E. LIPSEY, Queens College, City University of New York, and National Bureau of Economic Research
The Behavior of National Price Levels
EDWARD LEAMER, University of California-Los Angeles
The Sensitivity of International Comparisons of Capital Stock Measures to Different "Real" Exchange Rates
Discussants: JAGDISH BHAGWATI, Columbia University
CHRISTOPHER CLAGUE, University of Maryland
DALE W. JORGENSON, Harvard University

2:30 P.M. SURPRISES FROM DEREGULATION
Presiding: MURRAY WEIDENBAUM, Washington University
Papers: ALFRED KAHN, Cornell University
Surprises from Airline Deregulation
ROBERT W. CRANDALL, The Brookings Institution
The Breakup of the Bell System
EDWARD KANE, Ohio State University
Intensifying Competition Among Financial Regulators
Discussants: GEORGE KAUFMAN, Loyola University of Chicago
MARVIN KOSTERS, American Enterprise Institute
PAUL W. MACAVOY, University of Rochester

2:30 P.M. NEAR-RATIONAL BEHAVIOR
Presiding: ROBERT J. SHILLER, Yale University
Papers: GEORGE A. AKERLOF AND JANET L. YELLEN, University of California-Berkeley
The Macroeconomic Applications of a Dynamic Envelope Theorem
LAURENCE BALL, New York University, AND DAVID ROMER, Princeton University
Imperfect Competition and Keynesian Economics
JOHN Y. CAMPBELL AND ALBERT S. KYLE, Princeton University
Heterogeneous Investors and the Behavior of Asset Prices
Discussants: JOHN B. TAYLOR, Stanford University
ROBERT E. HALL, Stanford University
DOUGLAS W. DIAMOND, Yale University

2:30 P.M. SEARCH BEHAVIOR IN LABOR AND PRODUCT MARKETS
Presiding: EDMUND S. PHELPS, Columbia University
Papers: JOSEPH E. STIGLITZ, Princeton University
Pareto Inefficiency of Search in Efficiency Wage Models
DENNIS J. SNOWER, University of London, AND STEVE ALPERN, London School of Economics
"High-Low" Search in Product Labor Markets
CHRISTOPHER PISSARIDES, London School of Economics
The Search Equilibrium Approach to Macroeconomics
ALLAN DRAZEN, Tel Aviv University
Self-Justifying Optimism in a Trade-Friction Model of the Business Cycle
Discussants: DALE T. MORTENSEN, Northwestern University
KENNETH BURDETT, Cornell University

2:30 P.M. GOVERNMENT INTERVENTION AND INTERNATIONAL ECONOMICS
Presiding: JAMES PECK, Northwestern University
Papers: JACOB PAROUSH, Bar-Ilan University, AND YORAM LANDSKRONER, Hebrew University
The Economics of Debt Relief
JOHN P. CASKEY, Washington University-St. Louis
Latin American Debt, Crisis Containment Mechanisms and the Role of the IMF
CLETUS C. COUGHLIN, University of Georgia, JOSEPH V. TERZA, Pennsylvania State University, AND NOOR AINI KHALIFAH, University of Georgia
The Determinants of Escape Clause Petitions
WYATT B. MANKIN, Consultant
The Optimum Quantity of Money in an Open Economy
Discussants: ERIC FISHER, Cornell University
AQUILES ALMANSI, University of Michigan
BARBARA CRAIG, Virginia Polytechnic Institute and State University
EUGENE YUN, University of British Columbia

2:30 P.M. THE ENTERPRISE UNDER SOCIALISM: EMPIRICAL STUDIES
Presiding: ED A. HEWETT, The Brookings Institution
Papers: JANEZ PRASNIKAR, University of Ljubljana, Yugoslavia, AND JAN SVEJNAR, University of Pittsburgh
Economic Performance of Yugoslav Labor-Managed Firms
MARTON TARDOS, Institute of Economics, Budapest
Empirical Research on the Hungarian Enterprise: A Summary of Recent Results
(TO BE ANNOUNCED), *Institute of Economics and Industrial Engineering*, Novosibirsk
Empirical Research on the Soviet Enterprise: A Summary of Recent Results
Discussants: JOSEF C. BRADA, Arizona State University
EVSEY D. DOMAR, Massachusetts Institute of Technology

2:30 P.M. THEORY OF FINANCIAL INSTITUTIONS
Presiding: LAWRENCE WHITE, Federal Home Loan Bank Board
Papers: PEKKA AHTIALA, University of Tampere, Finland
The Bank as a Business Firm: Causes and Consequences of Customer Specific Capital
ELAZAR BERKOVITCH, University of Michigan, AND STUART I. GREENBAUM, Northwestern University
The Loan Commitment as an Optimal Financing Contract
DAVID I. FAND, Wayne State University
Monetary Reform
DEBORAH LUCAS AND ROBERT MCDONALD, Northwestern University
Valuing Bank Liabilities with Incomplete Information About Loan Quality
Discussants: LAWRENCE BENVENISTE, Board of Governors of the Federal Reserve System and Northwestern University
ANTHONY SAUNDERS, New York University
V. V. CHARI, Federal Reserve Bank of Minneapolis
YUK SHEE CHAN, University of Southern California

2:30 P.M. ALVIN HARVEY HANSEN: AN ANNIVERSARY REMEMBRANCE AND APPRECIATION
Presiding: JOHN KENNETH GALBRAITH, Harvard University
Panel: RICHARD A. MUSGRAVE, University of California-Santa Cruz
WALLACE C. PETERSON, University of Nebraska
HENRY S. REUSS, Former Chair, Joint Economic Committee
WALTER S. SALANT, The Brookings Institution
JAMES TOBIN, Yale University

2:30 P.M. MACRO MODELS AND POLICIES IN ASIAN COUNTRIES (Joint Session with the American Committee on Asian Economic Studies)
Presiding: JANG H. YOO, Virginia Commonwealth University
Papers: STEVEN L. GREEN, Baylor University
Currency Substitution and the Distributional Effects of Monetary Policy in Korea
KANHAYA L. GUPTA, University of Alberta
Interest Rates, Inflation, Supply Shocks, and Structuralism in Asian Countries: A Comparative Study
BENJAMIN J. KIM, University of Nebraska-Lincoln
Variability of Money, Price, and Output in the Japanese Economy
PARTHASARATHI SHOME, International Monetary Fund
On the Elasticity of Developing Country Tax Systems
Discussants: RICHARD F. KOSOBUD, University of Illinois-Chicago
MOSHIN S. KHAN, International Monetary Fund
LAWRENCE B. KRAUSE, University of California-San Diego

2:30 P.M. AN ECONOMIC ASSESSMENT OF THE REAGAN YEARS WITH DIFFERENT PARADIGMS
Presiding: PAUL DAVIDSON, University of Tennessee
Papers: ALLAN H. MELTZER, Carnegie-Mellon University
Policies and Politics of the Reagan Administration Years
ALFRED E. EICHNER, Rutgers University
A Post-Keynesian View of the Reagan Years
MARC MILES, H. C. Wainwright & Co.
An Incentive (Supply Side) Analysis of the Reagan Years
SAMUEL BOWLES, University of Massachusetts, DAVID M. GORDON, New School for Social Research, AND THOMAS E. WEISSKOPF, University of Michigan
The Failure and Contradictions of Reaganomics
Discussants: LEONARD SILK, *The New York Times*
JAMES K. GALBRAITH, University of Texas-Austin

2:30 P.M. THE SOUTH AFRICAN ECONOMY AND THE IMPACT OF ECONOMIC SANCTIONS
Presiding: CHARLES M. BECKER, Economics Institute
Papers: RONALD FINDLAY, Columbia University, AND MATS LUNDAHL, Lund University
Racial Discrimination, Dualistic Labor Markets and Foreign Investment
WILLIAM KAEMPFER, University of Colorado, AND ANTON LOWENBERG, California State University-Northridge
The Political Economy of Apartheid and Oil Sanctions

CHARLES M. BECKER, Economics Institute
Investment and Divestment Policies in South Africa and Its Periphery
Discussants: RAY MARSHALL, University of Texas-Austin
THOMAS WILLETT, Claremont Graduate School
EDWARD TOWER, Duke University

2:30 P.M. FEDERAL GOVERNMENT CAPITAL BUDGETING
Presiding: ALICE M. RIVLIN, The Brookings Institution
Papers: MICHAEL J. BOSKIN, Stanford University
Recent Studies of Government Capital Accounts and Their Use in Interpreting the Short- and Long-Run Implications of Fiscal Policy
CHARLES HULTEN, University of Maryland
Issues in Measuring Depreciation and Obsolescence of Government Capital
PAUL PIEPER, University of Illinois-Chicago
Government Capital Budgeting Under Alternative Assumptions on the Relevance of Tax vs. Debt Finance
Discussants: JAMES M. POTERBA, Massachusetts Institute of Technology
EUGENE KROCH, University of Pennsylvania
CHARLES L. SCHULTZE, The Brookings Institution

2:30 P.M. ECONOMICS OF PROPERTY RIGHTS: INTERNATIONAL PERSPECTIVES (Joint Session with the Association for Comparative Economic Studies)
Presiding: JOHN H. MOORE, National Science Foundation
Papers: SVETOZAR PEJOVICH, Texas A&M University
Property Rights and Innovation
GERTRUDE SCHROEDER GREENSLADE, University of Virginia
Property Rights Issues in Economic Reforms in Communist Countries
S. FRED SINGER, George Mason University
Global Environmental Change and Property Rights
Discussants: (TO BE ANNOUNCED)

2:30 P.M. IMMIGRANTS' ROLE IN THE LABOR MARKET: VIEWS FROM URBAN, AGRICULTURE, AND POLICY PERSPECTIVES (Joint Session with the Industrial Relations Research Association)
Presiding: KEVIN F. MCCARTHY, Rand Corporation
Papers: BARRY R. CHISWICK, University of Illinois-Chicago
Implications of Immigration Reform for Immigrants' Skills
THOMAS J. ESPENSHADE, Brown University, AND J. EDWARD TAYLOR, Urban Institute
Immigration Reform and Supply of Labor to Agriculture
ANNELISE ANDERSON, Hoover Institution, Stanford University
Policy Issues in Regulation of Immigrants in the Labor Market
LAWRENCE P. BRUNNER, Central Michigan University
Who Benefits from U.S. Immigration?
Discussant: MICHAEL J. GREENWOOD, University of Colorado

2:30 P.M. CONFLICT AND PEACE ECONOMICS II: MILITARY EXPENDITURES AND WORLD SYSTEM MODELING (Joint Session with the Peace Science Society (International))
Presiding: (TO BE ANNOUNCED)
Papers: SHERMAN ROBINSON AND LAURA D. TYSON, University of California-Berkeley
General Equilibrium Analysis of the Impact of Changes in Military Expenditures
CHRISTINE SMITH, Griffith University, Australia
Comparison of Computable General Equilibrium and Other Approaches to World System Modeling
WALTER ISARD, Cornell University
Military Expenditures in World System Modeling
Discussants: FAYE DUCHIN, New York University
F. GERARD ADAMS, University of Pennsylvania

2:30 P.M. 1987 DISTINGUISHED LECTURE ON ECONOMICS IN GOVERNMENT (Joint Session with the Society of Government Economists)
Presiding: ROBERT H. ATEN, U.S. Department of the Treasury
Speaker: MARTIN FELDSTEIN, Harvard University and National Bureau of Economic Research
Thinking About International Economic Cooperation

2:30 P.M. LATIN MIGRATION TO THE U.S. AND THE LATIN AMERICAN ECONOMIC CRISIS (Joint Session with the Hispanic Professors of Economics and Business)
Presiding: JORGE SALAZAR-CARRILLO, Florida International University
Papers: GEORGE BORJAS, University of California-Santa Barbara
Recent Mexican-American Migration and Economic Conditions in Mexico
ANTONIA JORGE, RAUL MONCARZ, AND JORGE SALAZAR-CARRILLO, Florida International University
The Latin Debt Crisis and Labor Emigration to the U.S.
ODED STARK, Harvard University
Migration, Return Migration and Family Remittances
SERGIO DIAZ-BRIQUETS, Duquesne University
Caribbean Migration to the U.S.: Economic or Political Causes?
Discussants: LUIS LOCAY, State University of New York
MARTA TIENDA, Stanford University
ROGER BETANCOURT, University of Maryland

2:30 P.M. THEORIES OF THE FIRM (Joint Session with the American Finance Association)
Presiding: ROBERT C. MERTON, Massachusetts Institute of Technology
Papers: OLIVER WILLIAMSON, University of Pennsylvania
MICHAEL JENSEN, University of Rochester
Discussant: ROBERT C. MERTON, Massachusetts Institute of Technology

4:45 P.M. BUDGET DEFICITS ONCE MORE (Roundtable)
Presiding: ALAN S. BLINDER, Princeton University
Papers: ROBERT EISNER, Northwestern University
Rhetoric and Reality
ALICE M. RIVLIN, The Brookings Institution
The Political Economy of Deficits
ALLEN SINAI, Shearson Lehman Bros. and New York University
Deficits, Expectations, and Financial Markets
JAMES TOBIN, Yale University
The Necessity of Increasing Taxes

8:00 P.M. RICHARD T. ELY LECTURE
Presiding: ROBERT EISNER, Northwestern University
Speaker: ALAN S. BLINDER, Princeton University
The Challenge of Unemployment

Tuesday, December 29, 1987

8:00 A.M. COMPUTER-BASED TECHNOLOGIES AND PRODUCTIVITY, EMPLOYMENT, AND TRADE
Presiding: MICHAEL J. PIORE, Massachusetts Institute of Technology
Papers: FAYE DUCHIN, New York University
Implications of Automation for American Trade and Jobs
PETER S. ALBIN, City University of New York, AND EILEEN APPELBAUM, Temple University
Work Organization and Productivity Change in Computer Rationalized Corporations
EDWARD N. WOLFF, New York University, AND DAVID R. HOWELL, New School for Social Research
Skill Changes in the Labor Force and Computer-Based Technologies, 1960–85
EILEEN L. COLLINS, National Science Foundation
Meeting the Scientific and Technical Staffing Requirements for Rationalization of the American Economy
Discussants: MARK ROBERTS, AFL/CIO
ROBERT BEDNARZIK, U.S. Department of Labor
ALAN FECHTER, National Academy of Sciences

8:00 A.M. U.S. TRADE AND PROTECTION
Presiding: JEFFREY FRANKEL, University of California-Berkeley
Papers: CATHERINE MANN, Board of Governors of the Federal Reserve System
Trade and Protection
ALBERTO GIOVANNINI, Columbia University and National Bureau of Economic Research
The Behavior of Traded Goods Prices

SUSAN COLLINS, Harvard University, AND KENNETH A. FROOT, Massachusetts Institute of Technology
Exchange Rates and U.S. Import Behavior
LUIS TELLEZ, Itam, Mexico
Title: (To Be Announced)
Discussants: RICHARD BALDWIN, Columbia University
ROBERT FLOOD, Northwestern University and International Monetary Fund

8:00 A.M. PREDATORY PRICING: A CRITIQUE OF THE NEW ORTHODOXY
Presiding: ALLEN R. FERGUSON, AFE, Inc.
Papers: MALCOLM BURNS, University of Kansas
Empirical Studies of Predatory Price Cutting: Second Thoughts and New Evidence
JOHN ROBERTS, Stanford University
Informational Asymmetry and Predatory Pricing
WILLIAM G. SHEPHERD, University of Massachusetts
Criteria for Anticompetitive Actions
Discussants: THOMAS GALE MOORE, Council of Economic Advisors
LAWRENCE WHITE, Federal Home Loan Bank Board

8:00 A.M. THE FEMINIZATION OF POVERTY
Presiding: JOHN GRAHAM, Rutgers University-Newark
Papers: THOMAS KNIESNER, University of North Carolina, MARJORIE MCELROY, Duke University, AND STEVEN WILCOX, University of North Carolina
Family Structure, Race, and the Feminization of Poverty
LAURIE BASSI, Georgetown University
Poverty Among Women and Children: What Accounts For the Change?
MARTHA HILL, University of Michigan, AND HEIDI HARTMANN, U.S. Bureau of the Census
Mother's Employment and Poverty Prevention
ANDREA BELLER, University of Illinois-Urbana, AND JOHN GRAHAM, Rutgers University-Newark
Child Support Receipts: Evidence from Repeated Cross Sections of the Current Population Survey
Discussants: IRWIN GARFINKEL, University of Wisconsin-Madison
ROBERT MOFFITT, Brown University

8:00 A.M. NEW DIRECTIONS IN MACROECONOMICS
Presiding: R. GLENN HUBBARD, Northwestern University
Papers: STEVEN FAZZARI AND JOHN CASKEY, Federal Reserve Bank of St. Louis
Price Flexibility and Macroeconomic Stability
ROBERT B. BARSKY AND JEFFREY A. MIRON, University of Michigan
The Seasonal Cycle and the Business Cycle
KNUT ANTON MORK, Vanderbilt University
Where's the Boom? The Oil Price Decline and the U.S. Economy in 1986: A Reassessment of the Theories of the 1970s
DAVID NICKERSON, University of British Columbia
Consistent Intertemporal Equilibria in Games of Macroeconomic Stabilization
Discussants: MARK WATSON, Northwestern University
JAMES A. WILSON, University of California-Berkeley

8:00 A.M. COLLECTIVE-CHOICE APPROACHES TO ECONOMICS
Presiding: BARRY R. WEINGAST, Washington University
Papers: JAGDISH BHAGWATI, Columbia University
The Political Economy of Foreign Investment
MANCUR OLSON, University of Maryland
Collective Choices and Sticky Prices
LESLIE YOUNG AND STEPHEN MAGEE, University of Texas-Austin
Black Hole Tariffs: Endogenous Redistribution in General Equilibrium
Discussants: THOMAS SCHELLING, Harvard University
GORDON TULLOCK, George Mason University
CHRISTOPHER CLAGUE, University of Maryland

8:00 A.M. UNCERTAINTY IN MACROECONOMICS
Presiding: ROBERT M. TOWNSEND, University of Chicago
Papers: CHRISTOPHER A. SIMS, University of Minnesota
Uncertainty Across Models
THOMAS J. SARGENT, University of Minnesota, AND ALBERT MARCET, Carnegie-Mellon University
Convergence of Least-Squares Learning to Rational Expectations
OLIVIER J. BLANCHARD, Massachusetts Institute of Technology, AND N. GREGORY MANKIW, Harvard University
Uncertainty and Future Commitments
GEORGE M. VON FURSTENBERG, Indiana University, AND ESFANDIAR MAASOUMI, University of California-Santa Barbara
Macroeconomic Implications of the Information Revolution
Discussants: EDWARD E. LEAMER, University of California-Los Angeles
MICHAEL WOODFORD, University of Chicago

8:00 A.M. THE ESTIMATION AND MEASUREMENT OF SPILLOVER EFFECTS OF *R&D* INVESTMENT
Presiding: PAUL A. DAVID, Stanford University
Papers: IAIN COCKBURN AND ZVI GRILICHES, Harvard University
Industry Effects and Appropriability Measures in the Market Valuation of *R&D* and Patents
RICHARD LEVIN, Yale University
Appropriability, *R&D* Spending, and Technological Performance
EDWIN MANSFIELD, University of Pennsylvania
The Speed and Cost of Industrial Innovation: External vs. Internal Technology
JEFFREY BERNSTEIN, Carleton University, AND M. ISHAQ NADIRI, New York University
Measurement of *R&D* Interindustry Spillover, Its Effect on the Structure of Production and the Social and Private Rates of Return
Discussants: ADAM JAFFE, Harvard University
PETER REISS, Stanford University
MARK SCHANKERMAN, New York University
WESLEY COHEN, Carnegie-Mellon University

8:00 A.M. IMPROVING THE DIALOGUE BETWEEN EMPIRICAL, THEORETICAL, AND POLICY RESEARCH: A PANEL DISCUSSION
Presiding: JOSEPH E. STIGLITZ, Princeton University
Panel: ROBERT E. HALL, Stanford University
F. THOMAS JUSTER, University of Michigan
RICHARD SCHMALENSEE, Massachusetts Institute of Technology
GEORGE A. AKERLOF, University of California-Berkeley
JOHN B. TAYLOR, Stanford University

8:00 A.M. ECONOMIC CAUSES AND CONSEQUENCES OF CORPORATE RESTRUCTURING (Joint Session with the Association of Managerial Economists)
Presiding: MARK HIRSCHEY, Rice University
Papers: GREG A. JARELL, University of Rochester, JEFFREY NETTER, Securities and Exchange Commission, AND JOHN A. POUND, Harvard University
Recapitalizations as a Takeover Defense: How Do Shareholders Fair?
KENNETH MARTIN, University of Iowa, CLAUDIO LODERER, Purdue University, AND JOHN J. MCCONNELL, Purdue University
Corporate Takeovers, Management Turnover, and Corporate Performance
RANDALL MORCK, University of Alberta, ANDREI SHLEIFER, Princeton University, AND ROBERT W. VISHNY, University of Chicago
Characteristics of Hostile and Friendly Takeover Targets
Discussants: SAM PELTZMAN, University of Chicago
ROBERT J. SHILLER, Yale University
DAVID H. MALMQUIST, Securities and Exchange Commission

8:00 A.M. MULTI-PRODUCT HEALTH CARE PRODUCTION AND COST FUNCTIONS (Joint Session with the Health Economics Research Organization)
Presiding: BARBARA H. KEHRER, Kaiser Family Foundation
Papers: WILLIAM S. CUSTER AND RICHARD J. WILLKE, American Medical Association
Teaching Hospital Costs: The Effects of Medical Staff Characteristics and a Changing Economic Environment

MARTIN GAYNOR, Center for Health Economics Research and National Bureau of Economic Research, AND MARK PAULY, University of Pennsylvania
Productive Efficiency in Medical Group Practice
W. BRUCE VOGEL AND KATHERINE R. JONES, University of Florida
Multiple-Output Production Efficiency in the Presence of Imperfectly Observed Outputs: The Case of ESRD Facilities
Discussants: RICHARD ARNOULD, University of Illinois-Urbana
PETER ROSSI, University of Chicago
RANDALL ELLIS, Boston University

8:00 A.M. IS IT MONEY OR CREDIT, OR BOTH, OR NEITHER?
Presiding: (TO BE ANNOUNCED)
Papers: BEN S. BERNANKE AND ALAN S. BLINDER, Princeton University
Monetary Transmission: Through Deposits or Credit?
BENJAMIN M. FRIEDMAN, Harvard University
Income and Money? Income and Credit?
ALLAN H. MELTZER, Carnegie-Mellon University, AND KARL BRUNNER, University of Rochester
Money and Intermediation
Discussants: CHARLES FREEDMAN, Bank of Canada
CHARLES I. PLOSSER, University of Rochester
ROBERT H. RASCHE, Michigan State University

8:00 A.M. ARE SOCIAL EXPERIMENTS A USEFUL POLICY TECHNIQUE? (Joint Session with the Industrial Relations Research Association)
Presiding: ROBERT SPIEGELMAN, W. E. Upjohn Institute for Employment Research
Papers: BURT BARNOW, ICF, Inc.
MICHAEL E. BORUS, Manpower Demonstration Research Corporation
RONALD EHRENBERG, Cornell University
ORLEY ASHENFELTER, Princeton University

8:00 A.M. ECONOMICS OF ISLAM AND ISLAMIZATION OF ECONOMICS (Joint Session with the Middle East Economic Association)
Presiding: ABBAS ALNASRAWI, University of Vermont
Papers: UDO STEINBACK, Deutsches Orient-Institut
Islamic Socioeconomic Debate and Policy: Majlis and Council of Experts in Review
SOHRAB BEHDAD, Denison University
State and Private Property Rights in Contemporary Shi-i Economic Thought: The Fundamental Dilemma of the Islamic Republic of Iran
HAMID HOSSEINI, King's College
Notions of Private Property in the Islamic Economics in Iran: A Review of Literature
Discussants: MANOUCHER PARVIN, University of Akron

8:00 A.M. ODE GRADUATE STUDENT PAPER SESSION (Joint Session with Omicron Delta Epsilon)
Presiding: ALEX J. KONDONASSIS, University of Oklahoma
Papers: CINDY ALEXANDER, University of California-Los Angeles
Ownership Structure and the Effect of Antitakeover Charter Amendments
ANDREAS MERIKAS, University of Oklahoma and Mississippi State University
Corporate Financing Behavior and How That Behavior Affects Security Returns
ELLEN JEAN SZARLETA, University of Wisconsin-Madison
The Impact of the Market and Socio-Market Economies Household Choice: A Case Study of Burkina Faso
Discussants: PATRICIA KRISTJANSON, University of Wisconsin-Madison
ATIF CEZAIRLI, Case Western Reserve University
SUNG PYO HONG, State University of New York-Stony Brook

8:00 A.M. CONFLICT AND PEACE ECONOMICS III. ANALYTICAL APPROACHES TO CONFLICT REDUCTION AND CONTROL (Joint Session with the Peace Science Society (International))
Presiding: WALTER ISARD, Cornell University
Papers: ROY RADNER, Bell Laboratories
A Model of Defense Protected Build-Down
ROBERT E. KUENNE, Princeton University
Conflict Resolution and the Theory of Mature Oligopoly
Discussants: MICHAEL INTRILIGATOR, University of California-Los Angeles
F. M. SCHERER, Swarthmore College

10:15 A.M. "THE NEW INDUSTRIAL STATE" AFTER TWENTY YEARS
Presiding: BERNARD SAFFRAN, Swarthmore College
Panel: JOHN KENNETH GALBRAITH, Harvard University
BARRY BLUESTONE, Boston College
ROBERT M. SOLOW, Massachusetts Institute of Technology
F. M. SCHERER, Swarthmore College

10:15 A.M. THE POLITICAL ECONOMY OF TERRORISM
Presiding: THOMAS C. SCHELLING, Harvard University
Papers: HARVEY E. LAPAN AND TODD SANDLER, Iowa State University
To Bargain or Not to Bargain, That Is the Question
DWIGHT R. LEE, University of Georgia
On the Optimal Retaliation Against Terrorists: The Problem of the Paid Riders
ERIC IKSOON IM AND JON CAULEY, University of Hawaii-Hilo
Intervention Policy Analysis of Skyjacking Incidents
Discussants: MANCUR OLSON, University of Maryland
JOHN TSCHIRHART, University of Wyoming

10:15 A.M. MICROECONOMIC DATA: CAN WE USE IT NOW THAT WE HAVE FOUND IT? EVIDENCE FROM THE LED
Presiding: ERNST R. BERNDT, Massachusetts Institute of Technology
Papers: FRANK M. GOLLOP, Boston College, AND JAMES L. MONAHAN, U.S. Bureau of the Census
From Homogeneity to Heterogeneity: An Index of Diversification
TIMOTHY DUNNE, MARK J. ROBERTS, AND LARRY SAMUELSON, Pennsylvania State University
Firm Entry Growth and Exit in U.S. Manufacturing
SANG V. NGUYEN, U.S. Bureau of the Census, AND EDWARD C. KOKKELENBERG, State University of New York-Binghamton
The Stock of Research and Development Knowledge and Multi-Factor Productivity Growth
FRANK R. LICHTENBERG, Columbia University
The Effects of *R&D* and Fixed Investment on Productivity
ROBERT H. MCGUCKIN, U.S. Bureau of the Census
The Efficiency of Mergers
Discussant: ROBERT D. WILLIG, Princeton University

10:15 A.M. ECONOMIC ASPECTS OF PRODUCT LIABILITY
Presiding: RICHARD ZECKHAUSER, Harvard University
Papers: RICHARD A. EPSTEIN, University of Chicago
The Political Economy of Product Liability Reform
PATRICIA DANZON, University of Pennsylvania
Insurance Aspects of Product Liability
W. KIP VISCUSI, Northwestern University
Product Liability and Regulation: Establishing the Appropriate Institutional Division of Labor
Discussants: JEROME CULP, Duke University
VICTOR GOLDBERG, Columbia University and Northwestern University
ROBERT W. CRANDALL, The Brookings Institution

10:15 A.M. THE ECONOMICS OF THE AGING OF THE BABY BOOM
Presiding: DAVID BLOOM, Harvard University
Papers: OLIVIA S. MITCHELL, Cornell University and National Bureau of Economic Research, AND PHILLIP LEVINE, Princeton University
The Baby Boom's Legacy: Relative Wages in the Twenty-First Century
JOYCE MANCHESTER, Dartmouth College and National Bureau of Economic Research
The Baby Boom, Housing, and Loanable Funds
TABITHA DOESCHER, Oklahoma State University, AND JOHN TURNER, U.S. Department of Labor
Social Security and the Baby Boom Generation
Discussants: DAVID BLOOM, Harvard University and National Bureau of Economic Research
JAMES POTERBA, Harvard University and National Bureau of Eonomic Research
JOHN HAMBOR, U.S. Social Security Administration

10:15 A.M. MONOPOLISTIC COMPETITION AND KEYNESIAN ECONOMICS
Presiding: ROBERT J. GORDON, Northwestern University
Papers: ROBERT E. HALL, Stanford University
A Noncompetitive, Equilibrium Model of Fluctuations

Julio Rotemberg, Massachusetts Institute of Technology, and Lawrence H. Summers, Harvard University
Procyclical Productivity with Monopolistic Competition
Ian Domowitz, R. Glenn Hubbard, and Bruce C. Petersen, Northwestern University
Market Structure, Price Flexibility, and the Business Cycle
Discussants: Matthew D. Shapiro, Yale University
Olivier J. Blanchard, Massachusetts Institute of Technology
Dennis W. Carlton, University of Chicago

10:15 a.m. Constitutional Convention Bicentennial
Presiding: Jonathan Hughes, Northwestern University
Papers: James Buchanan, George Mason University
The Economy and Constitutional Structure
Harry Scheiber, University of California-Berkeley
The Constitution and Individual Liberty
Samuel Bowles and Herbert Gintis, University of Massachusetts-Amherst
Jefferson's Hope, Tocqueville's Fear: Democracy and Capitalism in Retrospect
Discussants: Richard Wagner, Florida State University
Nathan Rosenberg, Stanford University

10:15 a.m. Electoral Influences on the U.S. Macroeconomy: Theory, Evidence, and Policy Options
Presiding: Thomas D. Willett, Claremont Graduate School
Papers: Alberto Alesina, Carnegie-Mellon University, and Jeffrey Sachs, Harvard University
Models of Electoral Influence on the U.S. Macroeconomy
Stephen E. Haynes and Joe A. Stone, University of Oregon
Empirical Evidence of Electoral Influence on the U.S. Macroeconomy
Thomas Mayer, University of California-Davis
Policy Options in a World with Political Influence on the U.S. Macroeconomy
Discussants: Christopher Ellis, University of Oregon
Kenneth Rogoff, University of Wisconsin-Madison
Guido Tabellini, University of California-Los Angeles

10:15 a.m. Advice for the Next President (Roundtable)
Presiding: James K. Galbraith, University of Texas-Austin
Panel: (To Be Announced)
Leon H. Keyserling, Conference on Economic Progress
Beryl W. Sprinkel, Council of Economic Advisors
Murray L. Weidenbaum, Washington University
Discussants: Henry S. Reuss, Former Chair, Joint Economic Committee
Michael J. Boskin, Standford University

10:15 a.m. Differing Theoretical Perspectives on the Household
Presiding: Nancy Folbre, University of Massachusetts
Papers: Elaine McCrate, University of Vermont
A Political-Economic Model of Gender Differences and Household Structure
Robert Pollak, University of Washington and University of Pennsylvania
A Transaction Cost Approach to Families: Parents and Children
Mark Rosenzweig, University of Minnesota
Risk, Private Information, and Household Structure
Discussants: Gary S. Becker, University of Chicago
Paula England, University of Texas-Dallas

10:15 a.m. Can Economic Forecasting Be Improved?
Presiding: Allen Sinai, Shearson Lehman Brothers
Papers: Lawrence R. Klein and Eduardo Sojo, University of Pennsylvania
Combinations of High and Low Frequency Data in Macroeconomic Models
Carol Corrado and Jane Haltmaier, Federal Reserve Board
The Use of High Frequency Data in Model-Based Forecasting at the Federal Reserve Board
E. Philip Howrey, Saul H. Hymans, and Michael R. Donihue, University of Michigan
Merging Monthly and Quarterly Forecasts: Experience with MQEM
Discussants: Clive W. J. Granger, University of California-San Diego
Frank de Leeuw, U.S. Department of Commerce

10:15 A.M. HISTORICAL PERSPECTIVES ON THE CONTRIBUTION OF FINANCIAL INSTITUTIONS TO ECONOMIC GROWTH
Presiding: LANCE DAVIS, California Institute of Technology
Papers: LARRY NEAL, University of Illinois-Urbana
War Finance and Industrialization: The British Experience During the Napoleonic Wars
CHARLES CALOMIRIS, Northwestern University, AND LARRY SCHWEIKART, University of Dayton
State Bank Regulation and Comparative Regional Development in Antebellum America
RICHARD SYLLA AND JACK W. WILSON, North Carolina State University
Financial Markets and the Great Merger Movement, 1895–1904
WILLIAM P. KENNEDY, London School of Economics
The Role of Financial Institutions in the Second Industrial Revolution: A Comparison of Britain, Germany, and the United States
Discussants: HUGH ROCKOFF, Rutgers University
DAVID WEIR, Yale University

10:15 A.M. FEMALE EXECUTIVES AND MIDDLE MANAGERS: EARNINGS AND LABOR SUPPLY
Presiding: CAROLYN SHAW BELL, Wellesley College
Papers: CATHERINE CANNING, University of Montreal
Earnings of Female and Male Middle Managers
ROBIN BARTLETT, Denison University, AND JANET GOULET, Wittenberg University
Hours Worked and Household Responsibilities of Executives
PHYLLIS WALLACE, MING-JE TANG, AND CATHLEEN K. TILNEY, Massachusetts Institute of Technology
Title: (TO BE ANNOUNCED)
Discussants: FRANK P. STAFFORD, University of Michigan
AUDREY FREEDMAN, The Conference Board
THOMAS JUSTER, University of Michigan

10:15 A.M. THE EFFECTS OF WORKER QUALITY, JOB QUALITY, AND JOB SENIORITY ON EARNINGS
Presiding: GARY ZARKIN, Duke University
Papers: KATHARINE G. ABRAHAM, University of Maryland, AND HENRY FARBER, Massachusetts Institute of Technology
Cross-Sectional Wage Variation: Are There Good Jobs, or Only Good Workers?
JOSEPH ALTONJI, Northwestern University
The Effects of Job Shopping, Experience, and Seniority on Wage Rates for Women
ROBERT TOPEL, University of Chicago
Search Specific Capital and Earnings
Discussants: MASINORI HASHIMOTO, Ohio State University
CHRISTOPHER FLINN, New York University
GARY ZARKIN, Duke University

10:15 A.M. DISCRIMINATION AND AFFIRMATIVE ACTION IN INTERNATIONAL PERSPECTIVE (Joint Session with the Association for Comparative Economic Studies)
Presiding: BARRY CHISWICK, University of Illinois-Chicago
Papers: WILLIAM BARTLETT, European University Institute, Italy
Yugoslavia
ZVI GITTELMAN, University of Michigan
Soviet Union
MATS LUNDAHL, University of Lund, Sweden
South Africa
MICHAEL WYZAN, Illinois State University
Malaysia
Discussants: BARRY CHISWICK, University of Illinois-Chicago
MARIANNE A. FERBER, University of Illinois-Urbana

10:15 A.M. ECONOMIC TRANSFORMATION AND BLACK EMPLOYMENT (Joint Session with the National Economic Association)
Presiding: RHONDA WILLIAMS, Yale University
Papers: HAROLD BARON, Mayor's Office, Chicago
Black Political Power and State Employment Policy: Emerging Strategies
ROSE BREWER, University of Minnesota
Economic Restructuring, Policy Changes, and the New Black Inequality
JOHN JEFFRES, Columbia University
Structural Change and State Economic Development: Implications for Black Americans
Discussant: RHONDA WILLIAMS, Yale University

10:15 A.M. ANALYSIS OF THE MILITARY SECTOR
Presiding: RONALD EHRENBERG, Cornell University
Papers: JAMES ANTON, State University of New York-Stony Brook, AND DENNIS YAO, University of Pennsylvania
Split Awards and Competition in Military Procurement
WALTER OI, University of Rochester
Wages in a Large Internal Labor Market: The Case of Military Compensation
JAMES HOSEK, Rand Corporation
Enlistment and Attrition Decisions
Discussants: ALMARIN PHILLIPS, University of Pennsylvania
ALINE QUESTER, Council of Economic Advisors
FINIS WELCH, UNICON Research Corporation

10:15 A.M. ODE CHAPTER ADVISERS AND REGIONAL DIRECTORS SESSION (Joint Session with Omicron Delta Epsilon)
Presiding: G. RANDOLPH RICE, Louisiana State University
Papers: T. P. BARBIERO AND A. A. BROWN, University of Windsor
The Role of Agriculture in Industrial Development: A Systemic Approach
F. NIROOMAND AND E. NISSAN, University of Southern Mississippi
Welfare Economic Analysis of Dualistic Development
W. J. SMITH AND M. B. VAUGHAN, Weber State College
An Economic Analysis of the Organ Transplant Market
M. T. DAL FRATELLO, Rockford College
Male-Female Wage Differentials in the Sunbelt Region
Discussants: F. R. GLAHE, University of Colorado-Boulder
A. C. KRAUTMANN, DePaul University

12:30 P.M. LUNCHEON IN HONOR OF NOBEL LAUREATE, JAMES BUCHANAN
Presiding: ROBERT EISNER, Northwestern University
Speaker: THOMAS E. BORCHERDING, Claremont Graduate School

2:30 P.M. ECONOMICS OF INSTITUTIONAL BEHAVIOR AND CHOICE
Presiding: RICHARD A. MUSGRAVE, University of California-Santa Cruz
Papers: DAVID BUTZ, University of California-Los Angeles, R. GLENN HUBBARD, Northwestern University, AND ROBERT WEINER, Brandeis University
Looking for Evidence of Opportunism: Contracting Behavior in the U.S. Natural Gas Industry
HENRY B. HANSMANN, Yale University
Ownership of Enterprise and Office of Tax Analysis
JERALD SCHIFF, Tulane University, AND BURTON A. WEISBROD, University of Wisconsin-Madison
A Model of Competition Between For-Profit and Nonprofit Organizations
Discussants: PAUL MACAVOY, University of Rochester
PETER CARSTENSEN, University of Wisconsin-Madison
RICHARD STEINBERG, Virginia Polytechnic Institute and State University

2:30 P.M. RENT SEEKING
Presiding: GORDON TULLOCK, George Mason University
Papers: JAGDISH BHAGWATI, Columbia University
Political Economy and Foreign Investment
MWANGI S. KIMENYI, University of Mississippi
The Cost of DUP and Rent-Seeking Activities
ROBERT D. TOLLISON, George Mason University
Unresolved Issues in the Theory of Rent Seeking
Discussants: DAVID S. COLANDER, Middlebury College
WARREN J. SAMUELS, Michigan State University

2:30 P.M. HARROD-DOMAR GROWTH MODELS AND STAGNATION REVISITED
Presiding: ROBERT EISNER, Northwestern University
Papers: HAROLD G. VATTER AND JOHN F. WALKER, Portland State University
Why Has the U.S. Economy Operated Chronically Below Potential?
W. ROBERT BRAZELTON, University of Missouri-Kansas City
Alvin H. Hansen: An American Interpreter of Keynes
RICHARD W. ENGLAND, University of New Hampshire
Harrod and Domar on Growth: A Sympathetic Interpretation

Discussants: Paul Davidson, University of Tennessee
Charles L. Schultze, The Brookings Institution
Evsey D. Domar, Massachusetts Institute of Technology

2:30 p.m. The Economic Progress of Racial and Ethnic Groups
Presiding: Glenn C. Loury, Harvard University
Papers: Barry R. Chiswick, University of Illinois-Chicago
The Labor Market Status of Hispanic Men Since the Turn of the Century
Harriett Orcutt Duleep, U.S. Commission on Civil Rights
Investments in Education by Asian and Hispanic Groups
James J. Heckman, University of Chicago
Impacts of the Economy and the State on South Carolina Blacks, 1940–80
James Cunningham, U.S. Commission on Civil Rights, and June Ellenoff O'Neill, Baruch College, City University of New York
Increasing Poverty of Minorities—Cycles or Trends?
Discussants: Ronald Oaxaca, University of Arizona
Peter Gottschalk, Boston College

2:30 p.m. The Widespread Depression Overseas: American and Pacific Influences
Presiding: Pentti J. K. Kouri, New York University
Papers: Jean-Paul Fitoussi, University of Paris
A Simple Model of an International Transmission Mechanism
Slobodan Djajic, Graduate Institute of International Studies, Geneva
Exchange Rates, Wages and the International Allocation of Capital
Edmund S. Phelps, Columbia University
World-Interest-Rate Shock in a Working Model of Slump and Recovery in Small Open Pure-Credit Economies
Discussants: Matthew B. Canzoneri, Georgetown University
John B. Taylor, Stanford University
Rudiger Dornbusch, Massachusetts Institute of Technology

2:30 p.m. The Rate of Profit in the Postwar U.S. Economy: Varied Perspectives
Presiding: Duncan K. Foley, Barnard College
Papers: Thomas Michl, Colgate University
New Evidence on the Falling Rate of Profit
Edward N. Wolff, New York University
Structural Change and the Decline of the Rate of Profit in the U.S. Economy
Fred Moseley, Colby College
The Decline of the Rate of Profit in the Postwar U.S. Economy: A Marxian Explanation
Discussants: Michael C. Lovell, Wesleyan University
Thomas E. Weisskopf, University of Michigan

2:30 p.m. Technological Innovation and Productivity Change in Japan and the United States
Presiding: Edward F. Denison, The Brookings Institution
Papers: Dale W. Jorgenson, Harvard University
Productivity and Economic Growth in Japan and the United States
Edwin Mansfield, University of Pennsylvania
Industrial Research and Innovation in Japan and the United States
Nathan Rosenberg and W. Edward Steinmueller, Stanford University
Why are Americans Such Poor Imitators?
Discussants: John W. Kendrick, George Washington University
J. Randolph Norsworthy, Rensselaer Polytechnic Institute
Rolf R. Piekarz, National Science Foundation

2:30 p.m. Worker-Controlled vs. Capital-Controlled Firms
Presiding: J. Michael Montias, Yale University
Papers: Masahiko Aoki, Stanford University and Kyoto University, Japan
The Relevance of the Theory of a Worker-Controlled Firm to the Understanding of a Capitalist Firm
Harvey Leibenstein, Harvard University
Alternative Organizational Forms and Hierarchy
Derek C. Jones, Hamilton College, and Jan Svejnar, University of Pittsburgh and Cornell University
Alternative Organizational Forms: Evidence of Economic Effects

JON BONIN, Wesleyan University, AND LOUIS PUTTERMAN, Brown University
Worker Reactions to Alternative Incentive Payment Systems
Discussants: J. MICHAEL MONTIAS, Yale University
LAURA TYSON, University of California-Berkeley
OLIVER HART, Massachusetts Institute of Technology

2:30 P.M. DISPLACED WORKERS: ECONOMICS AND POLICIES
Presiding: SHERWIN ROSEN, University of Chicago and National Bureau of Economic Research
Papers: ANDERS BJORKLUND, University of Stockholm, Sweden, AND BERTIL HOLMLUND, University of Uppsala, Sweden
The Swedish Approach to Worker Displacement
RONALD EHRENBERG, Cornell University and National Bureau of Economic Research
Advance Notice Provisions and Plant Closing Legislation
DANIEL HAMERMESH, Michigan State University and National Bureau of Economic Research
The Worker Displacement Problem in the United States
Discussants: JANICE FANNING MADDEN, University of Pennsylvania
ROBERT MCKERSIE, Massachusetts Institute of Technology
SHERWIN ROSEN, University of Chicago and National Bureau of Economic Research

2:30 P.M. CHALLENGING SOME CONVENTIONAL WISDOMS ABOUT THE LABOR MARKET
Presiding: KATHARINE G. ABRAHAM, The Brookings Institution
Papers: DAVID M. GORDON, New School for Social Research
The Un-Natural Rate of Unemployment
BARRY BLUESTONE, University of Massachusetts-Boston, AND BENNETT HARRISON, Massachusetts Institute of Technology
Structural Determinants of the Growth in Low-Wage Employment, 1963–85
WILLIAM T. DICKENS, University of California-Berkeley, AND KEVIN LANG, Boston University
The Re-emergence of the Segmented Labor Market Perspective
Discussants: ORLEY ASHENFELTER, Princeton University
DAVID BLOOM, Harvard University
ROBERT TOPEL, University of Chicago

2:30 P.M. THE CURRENT-ACCOUNT DEFICIT AND ITS ECONOMIC SIGNIFICANCE (Panel Discussion)
Presiding: ROBERT J. BARRO, Harvard University
Panel: MARTIN FELDSTEIN, Harvard University and National Bureau of Economic Research
ARTHUR LAFFER, A. B. Laffer Associates
JEFFREY SACHS, Harvard University
ALAN STOCKMAN, University of Rochester

2:30 P.M. THE THEORY OF SALES AND PRICING
Presiding: GAREY RAMEY, University of California-San Diego
Papers: BIRGER WERNERFELT, Northwestern University
Pricing When Buyers Develop Switching Costs
KEN JUDD, Northwestern University, AND MICHAEL RIORDAN, Stanford University
Price Signals of Quality with Consumer Learning
ROBERT GERTNER, University of Chicago
Inflation and Monopoly Power in a Duopoly with Search
PETER PASHIGIAN, University of Chicago
Demand Uncertainty and Sales: A Study of Fashion and Markdown Pricing
Discussants: NANCY LUTZ, Yale University
(To Be Announced)

2:30 P.M. MERGERS: CAUSES, EFFECTS, AND POLICY
Presiding: MARTIN K. PERRY, Bell Communications Research
Papers: EDGAR NORTON, Liberty University
The Motivations for Conglomerate Merger
RAPHAEL AMIT, Northwestern University
Efficient Conglomerate Diversification: Business Cycles and Corporate Performance
GEORGE BITTLINGMAYER, Washington University and International Institute for Management Federal Republic of Germany, AND VALERIE CHEH, Washington University-St. Louis
The Political Economy of Federal Merger Policy, 1950–84
Discussants: BRUCE PETERSEN, Northwestern University
DAVID RAVENSCRAFT, Federal Trade Commission

2:30 P.M. MARKETS FOR INFORMATION
Presiding: BETH ALLEN, University of Pennsylvania
Papers: ANAT ADMATI AND PAUL PFLEIDERER, Stanford University
Monopolistic Markets for Information
BETH ALLEN AND COSTAS AZARIADIS, University of Pennsylvania
Informational Theories of Employment
TARA VISHWANATH, Northwestern University
Parallel Search and Information Gathering
Discussants: DOUGLAS W. DIAMOND, University of Chicago and Yale University
BOYAN JOVANOVIC, New York University
SHELLY LUNDBERG, University of Washington

2:30 P.M. HOW PEOPLE REALLY BEHAVE
Presiding: THOMAS C. SCHELLING, Harvard University
Papers: DANIEL KAHNEMAN, University of California-Berkeley, AND RICHARD THALER, Cornell University
How Much Rationality Does Economics Need?
ALVIN ROTH, University of Pittsburgh
Lessons from Experimental Economics
THOMAS JUSTER AND JAMES F. MORGAN, University of Michigan
Title: (TO BE ANNOUNCED)
Discussants: THOMAS C. SCHELLING, Harvard University
VERNON L. SMITH, University of Arizona
STANLEY FISCHER, Massachusetts Institute of Technology

2:30 P.M. ASSESSMENT OF NATURAL RESOURCE DAMAGES UNDER CERCLA (Joint Session with the Association for Environmental and Resource Economists)
Presiding: GARDNER BROWN, JR., University of Washington
Papers: JOHN J. BOLAND, Johns Hopkins University, AND JEROME W. MILLIMAN, University of Florida
Overview of CERCLA Natural Resource Damage Procedures: Uses and Misuses of Economic Theory
A. MYRICK FREEMAN, Bowdoin College
Assessing Damages to Marine Resources: PCBs in the New Bedford Harbor
WILLIAM D. SCHULZE, University of Colorado, AND ROBERT D. ROWE, Energy and Resource Consultants
Natural Resources Damages in the Colorado Mountains: The Eagle Mine Case
Discussants: WILLIAM H. DESVOUGES, Research Triangle Institute
CHARLES J. CICCHETTI, National Economic Research Associates
RALPH D'ARGE, University of Wyoming

2:30 P.M. HIGH SCHOOL ECONOMICS: IMPLICATIONS FOR COLLEGE INSTRUCTION
Presiding: PHILLIP H. SAUNDERS, Indiana University
Papers: WILLIAM B. WALSTAD, University of Nebraska-Lincoln, AND JOHN C. SOPER, John Carroll University
Report Card on the Economic Literacy of U.S. High School Students
WILLIAM J. BAUMOL, Princeton University, AND ROBERT J. HIGHSMITH, Joint Council on Economic Education
The Economic Education of High School Seniors: Preliminary Findings from a New Data Base
STEPHEN G. BUCKLES, University of Missouri-Columbus, AND JOHN S. MORTON, Governors State University
The Effects of Advanced Placement on College Introductory Economics Courses
Discussants: RICHARD C. PORTER, University of Michigan
GEORGE H. BORTS, Brown University
DONALD N. MCCLOSKEY, University of Iowa

2:30 P.M. EFFICIENCY WAGES, LABOR RELATIONS, AND FULL EMPLOYMENT (Joint Session with the Union for Radical Political Economics)
Presiding: ROBERT M. SOLOW, Massachusetts Institute of Technology
Papers: LAWRENCE H. SUMMERS, Harvard University
Efficiency Wages and Cyclical Fluctuations
JAMES B. REBITZER, University of Texas-Austin and Hoover Institution
Unemployment, Labor Relations and Unit Labor Costs: A Study of U.S. Manufacturing Industries

SAMUEL BOWLES, University of Massachusetts-Amherst, AND ROBERT BOYER, Centre d'Etudes Prospectives d'Economie Mathematique Appliquees a la Planification, Paris
Labor Relations and Macroeconomic Theory
Discussants: EDWARD LAZEAR, University of Chicago and Hoover Institution
JULIET SCHOR, Harvard University

4:45 P.M. PRESIDENTIAL ADDRESS AND BUSINESS MEETING
Presiding: ROBERT EISNER, Northwestern University
Speaker: GARY S. BECKER, University of Chicago
Title: (TO BE ANNOUNCED)

Wednesday, December 30, 1987

8:00 A.M. THEORY AND APPLICATION: GAMES, AUCTIONS, AND OTHER MATTERS
Presiding: KYLE BAGWELL, Northwestern University
Papers: BERNARD C. BEAUDREAU, Lakehead University
The National Industrial Recovery Act: A Game Theoretical Interpretation
STUART E. THIEL, Washington State University
Auction Theory and the Market for Collectibles
GREGORY K. DOW, University of Alberta
The Genesis of Authority Relations: A Repeated Game Approach
S. Y. WU, University of Iowa
Entrepreneurs and the Economy with Both For-Profit and Not-for-Profit Producers
Discussants: CHARLES CALOMIRIS, Northwestern University
STEVE WILLIAMS, Northwestern University

8:00 A.M. THE MEASURE AND CHARACTER OF AMERICAN UNEMPLOYMENT
Presiding: (TO BE ANNOUNCED)
Papers: JANET NORWOOD, U.S. Bureau of Labor Statistics
Measuring Unemployment
RICHARD KRASHEVSKI, AFL-CIO
What's So Natural About High Unemployment?
RICHARD FREEMAN, Harvard University
Why Do Europeans Think the United States Has No "Unemployment" Problem?
Discussants: DANIEL HAMERMESH, Michigan State University
DONALD NICHOLS, University of Wisconsin-Madison
ROBERT M. SOLOW, Massachusetts Institute of Technology

8:00 A.M. ARE EXCHANGE RATE MODELS USEFUL?
Presiding: ROBERT P. FLOOD, Northwestern University and International Monetary Fund
Papers: ROBERT J. HODRICK, Northwestern University
Exchange Rate Determination: New Directions for Empirical Research
JEFFREY A. FRANKEL, University of California-Berkeley
The Magnitude of the Risk Premium, Continued: Implications of Variation in the Conditional Variance
JAMES M. BOUGHTON, International Monetary Fund
Exchange Rates and the Term Structure of Interest Rates
Discussants: KENNETH A. FROOT, Massachusetts Institute of Technology
KENNETH S. ROGOFF, University of Wisconsin-Madison
DAVID BACKUS, Queen's University

8:00 A.M. URBANIZATION AND INTERNAL MIGRATION
Presiding: ALAN M. SCHLOTTMANN, University of Tennessee
Papers: ODED STARK, Harvard University
Migrants and Markets
WILLIAM E. COLE, University of Tennessee
Internal Migration and the Informal Sector
MICHAEL J. GREENWOOD, University of Colorado
Migration and Urbanization in the Third World: Insights from the U.S. Experience
Discussants: CARMEL U. CHISWICK, University of Illinois-Chicago
ALAN M. SCHLOTTMANN, University of Tennessee
MICHAEL LIPTON, University of Sussex, England

8:00 A.M. THE SOURCES OF RESTRICTIVE MONETARY POLICY
Presiding: JAMES TOBIN, Yale University
Papers: GERALD A. EPSTEIN, University of Massachusetts-Amherst, AND JULIET B. SCHOR, Harvard University
Corporate Profitability as a Determinant of Restrictive Monetary Policy: Estimates for the Postwar United States
RAY C. FAIR, Yale University
Fed Behavior and the Optimal Choice of Monetary Instruments
STANLEY W. BLACK, University of North Carolina
Accounting for the Slowdown of Inflation in OECD Countries in the 1980s: How Much Is Due to Monetary Policy?
Discussants: BENJAMIN M. FRIEDMAN, Harvard University
THOMAS FERGUSON, University of Texas-Austin

8:00 A.M. ROUNDTABLE ON ALTERNATIVE POLICIES TOWARD THE STRUCTURALLY UNEMPLOYED
Presiding: DAVID M. GORDON, New School for Social Research
Papers: HOWARD STANBACK, Mayor's Office of Employment & Training, City of New York
The Black "Underclass": The Case for Public Intervention
CHARLES MURRAY, Manhattan Institute for Policy Research
The Black "Underclass": The Case Against Public Intervention
PAUL OSTERMAN, Massachusetts Institute of Technology
Displaced Workers: The New Structural Unemployment and the Possibility for Active Employment Policy
MARVIN KOSTERS, American Enterprise Institute
Displaced Workers: Adjustment Assistance in Perspective

8:00 A.M. EMPLOYMENT HOURS TRADEOFFS OVER THE BUSINESS CYCLE
Presiding: JOSEPH ALTONJI, Northwestern University
Papers: BRUCE HORNING, University of Minnesota
Intangible Capital and the Hoarding of Labor
THOMAS COLEMAN, State University of New York-Stony Brook
Employment and Hours in the United States
MICHAEL PERGAMIT, U.S. Bureau of Labor Statistics
Establishment Employment and Hours Movements Over the Business Cycle
Discussants: GARY HANSEN, University of California-Santa Barbara
EDWARD PRESCOTT, University of Minnesota
TIM DUNNE, Pennsylvania State University

8:00 A.M. THEORETICAL CONCEPTS AND EMPIRICAL MEASUREMENTS IN ECONOMICS
Presiding: ALICE G. NAKAMURA, University of Alberta
Papers: EDWARD N. WOLFF, New York University
Methodological Issues in the Measurement of Retirement Wealth
NANCY D. RUGGLES AND RICHARD RUGGLES, Yale University
Saving and Capital Formation: Concepts and Measurement
WILLIAM N. PARKER, Yale University
Theoretical Concepts and Measurement in Economic History
Discussants: F. THOMAS JUSTER, University of Michigan
STANLEY LEBERGOTT, Wesleyan University

8:00 A.M. MEASURING THE EFFECTS OF REGULATION
Presiding: DAVID BUTZ, University of California-Los Angeles
Papers: ROBIN A. PRAGER, Vanderbilt University
Using Stock Price Data to Measure the Effects of Regulation: The Interstate Commerce Act and the Railroad Industry
PHILIP ISRAILEVICH, Federal Reserve Bank of Chicago, AND KIM J. KOWALEWSKI, Federal Reserve Bank of Cleveland
The Impact of Regulation on Electric Utilities: A Test of Averch-Johnson and Joskow Views
JONI HERSCH, University of Oregon
EEO Law and Firm Profitability
Discussants: RAPHAEL AMIT, Northwestern University
ROBERT PORTER, Bell Communications Research

8:00 A.M. RATIONALITY AND CONSISTENCY OF CHOICE UNDER UNCERTAINTY
Presiding: DALE T. MORTENSEN, Northwestern University
Papers: WILLIAM SAMUELSON, Boston University, AND RICHARD ZECKHAUSER, Harvard University
Status Quo Bias in Individual Decision Making
MARK J. MACHINA, University of California-San Diego
Dynamically Consistent Non-Expected Utility Maximizers
W. KIP VISCUSI, Northwestern University
Prospective Reference Theory: Resolving the Paradoxes
Discussants: HOWARD KUNREUTHER, University of Pennsylvania
ROGER MYERSON, Northwestern University
DANIEL KAHNEMAN, University of California-Berkeley

8:00 A.M. U.S.–ASIAN TRADE RELATIONS: JAPAN (Joint Session with the American Committee on Asian Economic Studies)
Presiding: MANORANJAN DUTTA, Rutgers University
Papers: ROBERT W. STAIGER, Stanford University, ALAN V. DEARDORFF, AND ROBERT M. STERN, University of Michigan
The Effects of Protection on the Factor Content of U.S. and Japanese Foreign Trade
MARCUS NOLAND, Institute for International Economics
An Econometric Model of the Volume of International Trade
GARY SAXONHOUSE, University of Michigan
Comparative Advantage and Structural Adaptation
Discussants: LAWRENCE B. KRAUSE, University of California-San Diego
JERRY THURSBY, Ohio State University

8:00 A.M. PRIVATE AGRICULTURAL MARKETS IN A SOCIALIST ECONOMY (Joint Session with the American Agricultural Economic Association and the Association for Comparative Economic Studies)
Presiding: ELIZABETH M. CLAYTON, University of Missouri-St. Louis
Papers: STEVEN HEDLUND, University of Lund, Sweden
Soviet Union: The Anomaly of Private Socialist Agriculture
LOUIS PUTTERMAN, Brown University
People's Republic of China: Systemic and Structural Change in a North China Township
BRIAN D'SILVA, U.S. Department of Agriculture
Sudan: Liberalization and Control in Agriculture
Discussants: KAREN MCCONNELL BROOKS, University of Minnesota
NICHOLAS LARDY, University of Washington
JOHN STRAUSS, Yale University

8:00 A.M. NEW APPROACHES TO EMPIRICAL RESEARCH ON INVENTORIES (Joint Session with the International Society for Inventory Research)
Presiding: F. OWEN IRVINE, Michigan State University
Papers: JAMES A. KAHN, University of Rochester
Some Theory and Evidence on Inventory Fluctuations
KENNETH D. WEST, Princeton University
Unfilled Orders and Production Smoothing
JOHN HALTIWANGER, University of Maryland, AND LOUIS J. MACCINI, Johns Hopkins University
Inventories and Temporary and Permanent Layoffs: An Econometric Analysis
VALERIE RAMEY, Stanford University
Inventories as Factors of Production: An Empirical Analysis by Stage of Processing
Discussants: MICHAEL C. LOVELL, Wesleyan University
TRYPHON KOLLINTZAS, University of Pittsburgh
JEFFREY MIRON, University of Michigan
MARTIN EICHENBAUM, Carnegie-Mellon University

8:00 A.M. STRUCTURAL CHANGE AND THE LONGER-TERM EVOLUTION ON THE WORLD ECONOMY (Joint Session with the Society for Policy Modeling)
Presiding: STANLEY J. LAWSON, St. John's University
Papers: GEORGE C. WANG, California State University-Dominguez Hills
The Changing Trade Pattern of the Pacific-Rim Countries on the World Economy
DOUGLAS O. WALKER, United Nations, AND STANLEY J. LAWSON, St. John's University
The Future Pattern of World Industry: Projections to the Year 2000

Jack Baranson, Illinois Institute of Technology
Impact of Global Change on International Business Strategy
Discussants: Dominick Salvatore, Fordham University
M. Northrup Buechner, St. John's University
Howard Ross, Baruch College, City University of New York

10:15 A.M. The Political Economy of Tax Reform: Why Did It Happen? (Panel Discussion)
Presiding: David Bradford, Princeton University
Panel: Alan Murray, *Wall Street Journal*
Richard Musgrave, University of California-Santa Cruz
Thomas Romer, Carnegie-Mellon University
Randall Weiss, U.S. Joint Committee on Taxation

10:15 A.M. Ideology, Methodology, and Outside Influences in Economics
Presiding: George Stigler, University of Chicago
Papers: George Borts, Brown University
Ideology and the Content of Economics Journals
Robert D. Willig, Princeton University
The Role of Special Interests in Economic Research: Lessons from the Legal Arena
Richard Carson, University of California-San Diego, and Peter Navarro, University of San Diego
Ideology and Interest Groups in the Dispute Over Natural Resource Damage Assessment: Economists Caught in a Methodological Crossfire
Discussants: Dennis W. Carlton, University of Chicago
Hendrik Houthakker, *Review of Economics and Statistics*, and Harvard University
George Tolley, University of Chicago

10:15 A.M. What Do We Know about Consumption and Saving, and What are the Implications for Fiscal Policy?
Presiding: Alan S. Blinder, Princeton University
Papers: Michael J. Boskin, Stanford University
Recent Research on Measuring and Explaining Saving and Consumption Behavior: Issues and Evidence
Laurence J. Kotlikoff, Boston University
Experimental Evidence on Whether Consumers Make the Kinds of Computations Assumed in Long-Run and Intergenerational Models of Consumption and Saving
James M. Poterba, Massachusetts Institute of Technology
Are Consumers Forward Looking? Evidence from Tax Changes
Discussants: John B. Shoven, Stanford University
David A. Wise, Harvard University
Angus S. Deaton, Princeton University

10:15 A.M. Government and Markets in LDCs' Agricultural Development
Presiding: Albert Fishlow, University of California-Berkeley
Papers: Anne O. Krueger, Duke University, Maurice Schiff, World Bank, and Alberto Valdes, International Food Policy Research Institute
Rural Food Procurement and Subsidized Urban Distribution: Welfare Consequences
Raaj Kumar Sah and T. N. Srinivasan, Yale University
Rural Credit Organizations: Objectives, Structure, and Incentives
Avishay Braverman, World Bank, and J. Luis Guasch, University of California-San Diego
Capital Markets and the Marketed Surplus in LDCs
Discussants: Jeffrey S. Hammer, World Bank
Mark J. Machina, University of California-San Diego
Jagdish Bhagwati, Columbia University
Julie L. Anderson, Stanford University

10:15 A.M. Automobile Safety Legislation
Presiding: Charles A. Lave, University of California-Irvine
Papers: Christopher Garbacz, University of Missouri-Rolla
The Effects of Japanese Automobile Safety Legislation
Robert W. Crandall, The Brookings Institution, and John D. Graham, Harvard University
Fuel Economy Standards, Automobile Weight and the Highway Fatality Rate

GLENN BLOMQUIST, University of Kentucky
Motorists' Use of Safety Equipment
Discussants: SAM PELTZMAN, University of Chicago
JOHN TRAPANI, University of Texas-Arlington
W. KIP VISCUSI, Northwestern University

10:15 A.M. THE ECONOMICS OF THE ARMS RACE
Presiding: KENNETH E. BOULDING, University of Colorado
Papers: WILLIAM G. SHEPHERD, University of Massachusetts
Inherent National Security and Optimal Arms Spending
SEYMOUR MELMAN, Columbia University
Economic Consequences of the Arms Race
THOMAS RIDDELL, Smith College
U.S. Military Power, the Terms of Trade, and the Profit Rate
Discussants: F. M. SCHERER, Swarthmore College
LLOYD J. DUMAS, University of Texas-Dallas
WALTER ADAMS, Michigan State University

10:15 A.M. HIGHER EDUCATION FINANCE
Presiding: BURTON A. WEISBROD, University of Wisconsin-Madison
Papers: SANDRA A. BAUM, Skidmore College, AND SAUL SCHWARTZ, Tufts University
Student Borrowers and the Growth of Educational Debt
MICHAEL S. MCPHERSON, Williams College
Student Aid as Institutional Aid
W. LEE HANSEN, University of Wisconsin-Madison
Quality and Equity Tradeoffs in Higher Education Finance
Discussants: STEPHEN A. HOENACK, University of Minnesota
WALTER W. MCMAHON, University of Illinois-Urbana
ALICE M. RIVLIN, The Brookings Institution

10:15 A.M. LDC DEBT: CRISIS AND ADJUSTMENT
Presiding: RUDIGER DORNBUSCH, Massachusetts Institute of Technology
Papers: BENJAMIN COHEN, Tufts University
The Political Economy of LDC Debts
JEREMY BULOW, Stanford University, AND KENNETH S. ROGOFF, University of Wisconsin-Madison
Renegotiating LDC Debts
MANUEL PASTOR, Occidental College
The Debt Crisis and Beyond: Orthodox, Radical, and Structuralist Views
MICHAEL DOOLEY, International Monetary Fund
Market Valuation of LDC Debt
Discussants: STANLEY FISCHER, Massachusetts Institute of Technology
JESUS SEADE, World Bank

10:15 A.M. WHY IS UNEMPLOYMENT SO HIGH IN EUROPE?
Presiding: CHARLES L. SCHULTZE, The Brookings Institution
Papers: OLIVIER J. BLANCHARD, Massachusetts Institute of Technology, AND LAWRENCE H. SUMMERS, Harvard University
Is There a Natural Rate of Unemployment?
ROBERT M. COEN, Northwestern University, AND BERT G. HICKMAN, Stanford University
Is European Unemployment Classical or Keynesian?
ANDREW GLYN, Corpus Christi College, United Kingdom, AND ROBERT ROWTHORN, Cambridge University, United Kingdom
West European Unemployment: Corporatism and Structural Change?
Discussants: JEFFREY D. SACHS, Harvard University
ROBERT M. SOLOW, Massachusetts Institute of Technology
THOMAS E. WEISSKOPF, University of Michigan

10:15 A.M. COMPARATIVE STRATEGIES FOR ECONOMIC REFORM
Presiding: ABRAM BERGSON, Harvard University
Papers: JANOS KORNAI, Harvard University and Institute of Economics, Budapest
Reform Strategy in Hungary
ABEL AGANBEGIAN, USSR Academy of Sciences
Reform Strategy in the USSR

Speaker: (TO BE ANNOUNCED)
Reform Strategy in the People's Republic of China
Discussants: ED A. HEWETT, The Brookings Institution
NICHOLAS LARDY, University of Washington
PAUL MARER, Indiana University

10:15 A.M. FORTY YEARS OF THE GATT: RETROSPECTIVE AND OUTLOOK
Presiding: RICHARD N. GARDNER, Columbia University
Papers: KENT JONES, Babson College
Selective Discrimination and the Future of the MFN Clause
LAWRENCE B. KRAUSE, University of California-San Diego
The GATT and LDCs: Issues for Negotiation
GEZA FEKETEKUTY, Office of U.S. Trade Representation
The GATT and Trade in Services
RICHARD BLACKHURST, GATT
Transparency and Surveillance Issues
Discussants: C. MICHAEL AHO, Council on Foreign Relations
KENNETH ABBOTT, Northwestern University
CLAUDE BARFIELD, American Enterprise Institute

10:15 A.M. EMPIRICAL LABOR STUDIES
Presiding: DALE T. MORTENSEN, Northwestern University
Papers: STEPHEN H. BELL, JOHN H. ENNS, AND LARRY L. ORR, Abt Associates
Effects of Job Training and Employment on Earnings and Welfare Benefits of AFDC Recipients: The AFDC Homemaker-Home Health Aide Demonstrations
DONALD R. DEERE, Texas A&M University, AND JEFFREY A. MIRON, University of Michigan
The Cross-Sectional Impact of Unemployment Insurance on Layoff Rates and Employment
ERICA L. GROSHEN, Federal Reserve Bank of Cleveland
Employment Spells and Age-Earnings Profiles in the Service Sector: Good Jobs or Bad?
KEVIN J. MURPHY, Oakland University
Contract Duration in Collective Bargaining Agreements
Discussants: DAN SULLIVAN, Northwestern University
ROBERT LALONDE, University of Chicago

10:15 A.M. WORKFARE: WELFARE REFORM IN THE 1980S
Presiding: MICHAEL H. MOSKOW, Premark International
Papers: ANNA KONDRATAS, U.S. Department of Agriculture
Workfare for Food Stamp Recipients: Evidence from Nineteen Projects
JUDY GUERON, Manpower Development Research Corporation
Workfare for AFDC Recipients: Results of Eleven Experiments
BRADLEY R. SCHILLER, American University
Caseload Dynamics in Workfare and Nonworkfare States
Discussants: GARY BURTLESS, The Brookings Institution
ROBERT HAVEMAN, University of Wisconsin-Madison
CARL DAHLMAN, U.S. Department of Health and Human Services

10:15 A.M. NEW KEYNESIAN ECONOMICS: FINANCIAL CONSTRAINTS AND ECONOMIC ACTIVITY
Presiding: SANFORD GROSSMAN, Princeton University
Papers: JOSEPH E. STIGLITZ, Princeton University
Role of Monetary and Fiscal Policy in the Presence of Equity and Credit Rationing
BRUCE GREENWALD, Bell Communications Research
Financial Market Imperfections, Risk Distribution, and Price Rigidities
ANDREW WEISS, Bell Communications Research and Boston University
Credit Rationing: Micro Foundations and Macroeconomic Consequences
Discussants: EDMUND S. PHELPS, Columbia University
ROBERT E. HALL, Stanford University
GEORGE A. AKERLOF, University of California-Berkeley

10:15 A.M. NEW APPROACHES IN AGRICULTURAL POLICY RESEARCH (Joint Session with the American Agricultural Economic Association)
Presiding: V. KERRY SMITH, North Carolina State University-Raleigh
Papers: J. ARNE HALLAM, Iowa State University
Evaluating Individuals' Welfare Gains: Traditional and New Approaches

RICHARD E. JUST, University of Maryland
Making Economic Welfare Analysis Useful in the Policy Process: Implications of the Public Choice Literature
STEVEN T. BUCCOLA, Oregon State University
Social Welfare and Interpersonal Utility Comparisons in Applied Policy Research
Discussants: JOHN R. STOLL, Texas A&M University
DAVID M. G. NEWBERY, Cambridge University, England
DAVID BIGMAN, Hebrew University of Jerusalem

10:15 A.M. THE COMPETITIVENESS OF NATURAL RESOURCE INDUSTRIES (Joint Session with the American Association for the Advancement of Science)
Presiding: DAVID GULLEY, Columbia University and Ecole des Mines, Paris
Papers: ALBERTA CHARNEY, University of Arizona, THOMAS OXFORD, Zellars-Williams Co., AND CAROL TAYLOR, University of Florida
An Integrated Modeling Technique for Evaluating the Impact of Severance Taxation on the Phosphate Industry
RICHARD NEWCOMB AND STANLEY REYNOLDS, University of Arizona
Shifting Spatial Patterns of Investment Decision Making in World Aluminum Markets
ADAM ROSE, West Virginia University
Technology and Employment in the American Coal Industry
CHARLES KOLSTAD, University of Illinois-Urbana, AND FRANK WOLAK, Stanford University
Commodity Import Policy as a Portfolio Choice Problem: An Application to Coal Trade
Discussants: JOHN TILTON, Colorado School of Mines
MARTIN ZIMMERMAN, University of Michigan

10:15 A.M. NOVEL FEATURES OF THE CHICAGO TRADITION (Joint Session with the History of Economics Society)
Presiding: DONALD WALKER, Indiana University of Pennsylvania
Papers: NICHOLAS BOLABKINS, Lehigh University
Schmoller, Veblen, and Chicago
DONALD DEWEY, Columbia University
Frank H. Knight and the Legacy of Chicago Price Theory
LAWRENCE S. MOSS, Babson College
Is There a Chicago View of Human Capital and Intellectual Property Rights?
Discussants: RONALD COASE, University of Chicago
RICHARD GONCE, Grand Valley State College
WARREN J. SAMUELS, Michigan State University

10:15 A.M. INCOME AND WEALTH IN THE BLACK COMMUNITY (Joint Session with the National Economic Association)
Presiding: ADOLPH REED, Yale University
Paper: ANDREW F. BRIMMER, Brimmer & Co. and University of Massachusetts
Income, Wealth, and Investment Behavior in the Black Community
Discussants: MARCUS ALEXIS, University of Illinois-Chicago
MARGARET SIMMS, Joint Center for Political Studies
WILLIAM A. DARITY, JR., University of North Carolina-Chapel Hill

10:15 A.M. THE U.S. AND WORLD ECONOMIC OUTLOOK: PROSPECTS FOR NON-INFLATIONARY FULL EMPLOYMENT (Roundtable) (Joint Session with the National Association of Business Economists)
Presiding: JAMES SMITH, University of Texas-Austin
Papers: MARTIN S. FELDSTEIN, Harvard University and National Bureau of Economic Research
The Response to America's Changing International Role
LAWRENCE R. KLEIN, University of Pennsylvania
Unemployment and Inflation Outlook: What Needs to Be Done?
ALLEN SINAI, Shearson Lehman Brothers and New York University
Unemployment and Inflation Prospects in the U.S. and World Economies

NOTES

The annual meeting of the American Economic Association will be held in Chicago, Illinois, December 28–30, 1987.

The Professional Placement Service will be located at Palmer House Hotel. It will be open from 10:00 A.M. to 5:00 P.M., December 27; 9:00 A.M. to 5:00 P.M., December 28; and 9:00 A.M. to 12:00 noon, December 30.

Members wishing to give papers or make suggestions for the program for the AEA meeting, New York, New York, December 28–30, 1988, are invited to write to Dr. Joseph A. Pechman, The Brookings Institution, Washington, D.C. 20036. To be considered for contributed sessions, abstracts of (noneconometric) papers must be received no later than February 1, 1988.

Nominations for candidates for Sloan Research Fellowships are due by September 15. Candidates must be members of the regular faculty at a college or university in the United States or Canada, and must be at an early stage of their research careers. For information, write Sloan Research Fellowships, Alfred P. Sloan Foundation, Suite 2550, 630 Fifth Avenue, New York, NY 10111.

The S. S. Huebner Foundation for Insurance Education of the University of Pennsylvania is sponsoring a research grants competition. Grants of up to $10,000 are designed to support research in the field of risk and insurance. Full-time faculty members at colleges and universities in the United States and Canada are eligible. Each applicant must hold a terminal degree as grants are not available to support dissertation research. To apply, submit a research proposal to Dr. J. David Cummins, Associate Director, S. S. Huebner Foundation, 3641 Locust Walk, University of Pennsylvania, Philadelphia, PA 19104–6218. The deadline is March 1. (Proposals should follow the format in the Foundation's Guidelines for Preparation of Proposals, available upon request to Dr. Cummins.)

The Council for International Exchange of Scholars (CIES) announces the 1988–89 Fulbright Scholar-in-Residence Program. Opportunities are available for American colleges and universities to host a visiting scholar from abroad for all or part of the 1988–89 academic year. The program provides round-trip travel for the grantee and, for full-year awards, one accompanying dependent; a monthly maintenance allowance; and incidental allowances for travel, books, and essential services. The host institution is expected to share some costs in the form of supplementary funding and in-kind support such as housing. The deadline for receipt of proposals is November 1, 1987. Detailed program guidelines and proposal forms may be requested from CIES, Eleven Dupont Circle, NW, Suite 300, Washington, D.C. 10036–1257 (telephone 202+939–5401).

The Committee on Scholarly Communication with the People's Republic of China announces the 1988–89 National Program for Scholarly Exchanges with the PRC. The Graduate and Research Programs offer opportunities to graduate students and scholars in social sciences and humanities to conduct long-term research in China. The Visiting Scholar Exchange Program is a reciprocal short-term program offered to Americans to lecture and conduct research in China, and invites nominations of Chinese scholars to visit the United States. The application deadlines are October 10, 1987. For applications, write CSCPPRC, National Academy of Sciences, 2101 Constitution Ave., NW, Washington, D.C. 20418 (telephone 202+334–2718).

Resources for the Future (RFF) is offering two resident fellowships in honor of Gilbert F. White, internationally known statesman of science, for professionals who wish to devote a year to scholarly work on social science or public policy problems in areas of natural resources, energy, or the environment. The 1988–89 Award is open to individuals who will have completed their doctoral requirements by beginning of 1988–89 academic year. Selection criteria include the proposed work program and the individual's potential to contribute to solution of natural resource, energy, or environmental problems. Stipend to be announced (it was $27,000 for 1987–88) plus office and support at RFF and $1,000 for moving or living expenses. The deadline is March 1, 1988. Contact John F. Ahearne, Resources for the Future, 1616 P St., N.W., Washington, D.C. 20036.

The recipients of the in-residence Gilbert F. White Fellowships for 1987–88 are Walter V. Reid, University of Washington, and Robert M. Schwab, University of Maryland.

The nineteenth National Convention of the American Association for the Advancement of Slavic Studies will take place November 5–8, 1987 in Boston at the Park Plaza Hotel. For information: AAASS, 128 Encina Commons, Stanford University, Stanford, CA 94305–6029 (telephone 415+723–9668).

The Social Science History Association will hold its thirteenth annual meeting November 3–6, 1988, at the Bismark Hotel in Chicago. Preliminary proposals for individual papers or entire sessions should be sent by October 16, 1987 (the deadline for final proposals is February 15, 1988) to Caroline B. Brettell, Family and Community History Center, The Newberry Library, 60 W. Walton, Chicago, IL 60610 (telephone 312+943-9090, Ext. 316).

The International School of Economic Research announces a workshop on "Alternative Approaches to Macroeconomics," November 1–10, 1987, at the Certosa di Pontignano, Siena. The workshop is intended for advanced graduate students and junior faculty members. Enrollment will be limited to 30 fellows. Room and board will be provided by the school but, in principle, fellows will pay their own travel. Applications, including a detailed curriculum vitae and two letters of reference, should be sent to The Secretary, International School of Economic Research, Faculty of Economics and Banking, University of Siena, 53100, Italy (telephone 39+577-49059). The application deadline is September 15, 1987.

The Social Philosophy & Policy Center, Bowling Green State University, announces its second in a series of three bicentennial conferences on the general theme of "Economic Rights and the Constitution." These conferences are a component of the National Endowment for the Humanities Special Initiative for the Bicentennial of the U.S. Constitution. "Liberty, Property, and Government: The Nation and The Constitution Grow Together," will be held October 22–24, 1987, at Bowling Green State University. For further information, contact Kory Tilgner, Assistant Project Director, Social Philosophy & Policy Center, Bowling Green State University, Bowling Green, OH 43403-0188 (telephone 419+372-2536).

The second annual session of the Mediterranean Summer School of Industrial Organization will be held in Cargese (Corsica) at the Institute of Scientific Studies, September 27 to October 3, 1987. It is organized by the Centre National de la Recherche Scientifique (G.R.E.C.O. N^0 20 d'Economie Industrielle-France). For full information, contact P. M. Romani; A. Torre, Latapses CNRS, 1, Av. Albert Einstein, Sophia Antipolis, 06560 Valbonne, France (telephone 93+95-42-28).

The New York State Economics Association will hold its annual convention on October 9–10, 1987, at the Rochester Institute of Technology. For further details, contact Dr. John Helmuth, College of Business, Rochester Institute of Technology, P.O. Box 9887, Rochester, NY 14623 (telephone 716+475-2718).

The eighth International Symposium on Forecasting will be held in Amsterdam, The Netherlands, June 12–15, 1988. For further information, registration materials, and program details, contact The Symposium Secretariat ISF 88, Europaplein 12, 1078 GZ Amsterdam (telephone 31+20-440807; telex 13499 RAICO nl; telefax 31+20-464469).

A conference on Regionalism: Concepts and Applications will be held October 1–3, 1987, at Hooper-Schaefer Fine Arts Center, Baylor University, Waco, Texas. For further information, contact Program for Regional Studies, Baylor University, CSB Box 696, Waco, TX 76798 (telephone 817+755-2190).

The Association for Evolutionary Economics offers annually up to two $1,000 prizes for the best essays on any aspect of Theoretical or Applied Institutional Economics. Essays must be no longer than 30 double-spaced typed pages. Send 4 copies of submissions to the Editor, *Journal of Economic Issues*, Department of Economics, California State University, Sacramento, CA 95819-2694.

Economists who are strongly oriented toward the humanities, who use humanistic methods in their research, and who will be participating in meetings held outside the United States, Mexico, and Canada, that are concerned with the humanistic aspects of their discipline are eligible to apply for small travel grants of the American council of Learned Societies. Finanacial assistance is limited to airfare between manor commercial airports and will not exceed one-half of projected economy-class fare. Social scientists and legal scholars who specialize in the history or philosophy of their disciplines are eligible if the meeting they wish to attend is so oriented. Applicants must hold a Ph.D. degree or its equivalent, and must be citizens or permanent residents of the United States. To be eligible, proposed meetings must be broadly international in sponsorship or particiation, or both. The deadlines for application to be received in the ACLS office are: meeting scheduled between July and October, March 1; for meetings scheduled between November and February, July 1; for meetings scheduled between March and June, November 1. Please request application forms by writing directly to the ACLS (Attention: Travel Grant Program), 228 East 45th Street, New York 10017, setting forth the name, dates, place, and sponsorship of the meeting, as well as a brief statement describing the nature of your proposed role in the meeting.

Call for Papers: The fourth Biennial Conference of the Economics of Mental Health will be held at the Asilomar Conference Center, Pacific Grove, CA, April 20–22, 1988. Short descriptions of papers of about 1000 words should be submitted by November 10, 1987. Include title, author(s) name, current position, affiliation, address and phone number on the cover sheet. The description should discuss research hypotheses, methods, data, and results. Financial support for the conference is being provided by the National Institute of Mental Health. NIMH will pay travel and expenses for authors (1 per paper) and discussants, and author(s) of accepted papers will receive an honorarium. Submit descriptions to Professor Richard G. Frank, Health Services and Development Center, The Johns Hopkins University, 624 N. Broadway, Baltimore, MD 21205 (telephone 301 + 955–6546). Authors will be notified of the program committee's decision by December 1, 1987. To participate as a discussant, send a letter to Professor Frank.

Call for Papers: The *Economics of Education Review* is a quarterly journal seeking manuscripts on the topic The National Role in Vocational Education, for a special issue to be published in 1989. Specific topics of interest include nationwide studies of the impact of federal aid on vocational education; impact of federal aid on special populations and programs; and trends in vocational education in developed and developing nations. Abstracts should be sent to Elchanan Cohn, Editor, *EER*, College of Business Administration, University of South Carolina, Columbia, SC 29208.

Call for Papers: The fourteenth Eastern Economic Association meeting will be held in Boston at the Boston Park Plaza, March 10–12, 1988. Program participants and program suggestions are solicited. Papers are sought in both theoretical and applied areas of economics. To present a paper, submit two copies of a one-page abstract, along with *JEL* classification and a $20 submission fee, by November 15, 1987, to EEA, University of Connecticut, U-63, Room 345E, 341 Mansfield Road, Storrs, CT 06268.

Call for Papers: *The New York Economic Review* is soliciting articles covering all fields of economic research. The journal recently expanded to a semi-annual format, and has a blind referee system. It is sponsored by the New York State Economics Association. To submit articles, send two copies to William O'Dean, Editor, *NYER*, Department of Economics and Business, State University College, Oneonta, NY 13820.

Call for Papers: The annual conference of the Business Association of Latin American Studies will be held in San Diego, California, February 18–20, 1988. The deadline for papers, abstracts, or panel submissions is November 1, 1987. Accepted papers will be published in the *Proceedings*. All submissions and further inquiries should be directed to Joan Anderson and Denise Dimon, Co-Program Chairs, University of San Diego, School of Business Administration, San Diego, CA 92110.

Call for Papers: *The Review of Austrian Economics* is an annual scholarly journal soliciting articles on Mises, Hayek, Menger, and the Austrian or praxeological school of thought. There are no submission fees nor maximum length requirements. Send to Editor, *RAE*, 322 Massachusetts Avenue N.E., Washington, DC 20002.

Call for Papers: *Studies in Economic Analysis* is a biannual, refereed, student-edited journal soliciting research articles. Submission fee is $4.00 for nonsubscribers. Submit manuscripts to or request complete format and style requirements from the Editors, *SEA*, Department of Economics, College of Business Administration, University of South Carolina, Columbia, SC 29208.

Call for Papers: The Association for Arid Lands Studies will hold its annual conference at the Radisson Hotel, Denver, Colorado, April 27–30, 1988. A 250-word abstract should be sent to Andrew Schoolmaster, AALS President-Elect, Department of Geography, North Texas State University, Denton, TX 76203, prior to November 30, 1987. Participants must register prior to January 5, 1988. For full information contact Idrys Traylor, Jr., Executive Director, International Center for Arid and Semiarid Land Studies, Box 4620, Texas Tech University, Lubbock, TX 79409–4620.

Call for Papers: Notre Dame University Press announces a new book series on ethics, economics, and business. The series welcomes submissions of both manuscripts and book proposals, and is designed to provide scholarly, graduate-level, book-length analyses of social and moral issues involving business or economics. Textbooks and conference proceedings are discouraged. All manuscripts or proposals should be mailed to Thomas Donaldson, General Editor: *Soundings*, Department of Philosophy, Loyola University of Chicago, 6525 N. Sheridan Road, Chicago, IL 60626.

Call for Papers: The Economics section of the Western Social Science Association solicits proposals for the April 27–30, 1988 meetings at the Radisson Hotel in Denver, Colorado. Submit paper proposals, discussants, and chairs before November 30, 1987. Send materials to Western Social Science Economics Association, Program Chair Charles J. Ellard, Department of Accounting and Economics, Pan American University, Edinburg, TX 78539–2999.

Call for Papers: *Research on Economic Inequality* is a new annual that will publish both theoretical and empirical papers concerned with the measurement and analysis of economic inequality. Volume 2 will emphasize papers utilizing data from the Luxembourg Income study that focus on international comparisons of economic inequality. Volume 3 papers will assess the impact of government programs on economic inequality. Papers on other topics will also be considered. The annual can handle papers longer than of usual journal length. For further information, contact D. J. Slottje, Editor, *REI*, Department of Economics, Southern Methodist University, Dallas, TX 75275.

Call for Papers: The twenty-fourth Missouri Valley Economic Association meeting will be held in St. Louis, Missouri, March 10–12, 1988, at the Marriott Hotel. Persons wishing to participate should contact Donald L. Bumpass, Department of Economics, Texas Tech University, Lubbock, TX 79409 (telephone 806 + 742–2201).

Deaths

Walter W. Heller, professor emeritus, University of Minnesota, June 15, 1987.

Melvin Lurie, professor of economics, University of Wisconsin-Milwaukee, May 2, 1987.

Gunnar Myrdal, Stockholm, Sweden, May 17, 1987.

Retirements

Ralph W. Pfouts: University of North Carolina-Chapel Hill, May 31, 1987.

Harold L. Wattel: professor emeritus, Hofstra University, August 31, 1986.

Promotions

Dennis A. Ahlburg: associate professor of industrial relations, University of Minnesota, September 1986.

Steven G. Allen: professor, department of economics and business, North Carolina State University, August 1987.

Robert A. Becker: professor of economics, Indiana University-Bloomington, July 1, 1987.

Robert W. Campbell: professor of economics, Indiana University-Bloomington, July 1, 1987.

William A. Darity, Jr.: professor of economics, University of North Carolina-Chapel Hill, July 1, 1987.

Animesh Ghoshal: professor of economics, DePaul University, September 1987.

Thomas Gottschang: associate professor of economics, College of the Holy Cross, September 1, 1987.

Thomas G. Grennes: professor, department of economics and business, North Carolina State University, August 1987.

Simon Hakim: professor of economics, Temple University, July 1, 1987.

A. J. Harrison: professor, department of economics, McMaster University, July 1987.

Mitchell Kellman: professor, department of economics, City College, City University of New York, September 1986.

W. A. Lewchuk: associate professor, department of economics, McMaster University, July 1987.

Michael S. Miller: associate professor of economics, DePaul University, September 1987.

Steven Pressman: associate professor of economics and finance, Monmouth College, July 1, 1987.

A. L. Robb: professor, department of economics, McMaster University, July 1987.

Michael K. Salemi: professor of economics, University of North Carolina-Chapel Hill, July 1, 1987.

David Schap: associate professor of economics, College of the Holy Cross, September 1, 1987.

M. R. Veall: associate professor, department of economics, McMaster University, July 1987.

James M. Walker: associate professor of economics, Indiana University-Bloomington, July 1, 1987.

Administrative Appointments

Arthur Kraft, West Virginia University: dean, School of Business, Rutgers University-New Brunswick, July 1, 1986.

James L. Murphy: director of summer sessions, University of North Carolina-Chapel Hill, July 1, 1987.

Steven Pressman: chairman, department of economics and finance, Monmouth College, July 1, 1987.

George E. Rossmiller: director, National Center for Food and Agricultural Policy of Resources for the Future, March 1987.

New Appointments

Chichur Chao: assistant professor of economics, department of economics and finance, University of Mississippi, August 11, 1987.

William F. Chappell, Clemson University: assistant professor of economics, department of economics and finance, University of Mississippi, August 11, 1987.

John R. Conlon: assistant professor of economics, department of economics and finance, University of Mississippi, August 11, 1987.

Sanghamitra (Mita) Das, University of Wisconsin-Milwaukee: assistant professor of economics, Indiana University-Bloomington, August 1987.

Satya P. Das, University of Wisconsin-Milwaukee: professor of economics, Indiana University-Bloomington, August 1987.

Sarah Duffy: assistant professor, department of economics, Eastern Michigan University, September 1, 1987.

Sharon Erenburg: assistant professor, department of economics, Eastern Michigan University, September 1, 1987.

James W. Hughes, State University of New York-Albany: assistant professor of economics, Amherst College, July 1, 1987.

Ellen Hughes-Cromwick, Council of Economic Advisors: assistant professor of economics, Trinity College, September 1, 1987.

Samson M. Kimenyi: assistant professor of economics, department of economics and finance, University of Mississippi, August 11, 1987.

Jerzy D. Konieczny, University of Toronto: lecturer, department of economics, Wilfrid Laurier University, July 1, 1987.

Kathryn Larson, Iowa State University: assistant professor of economics, Elon College, August 1987.

Jia-yuan Lin, Tiffin University: assistant professor, division of business and accountancy, Northeast Missouri State University, September 1986.

Christina Liu, University of Chicago: assistant professor of economics and finance, Baruch College, City University of New York, August 1987.

Matthew R. Lynde, University of California-Berkeley: assistant professor of economics and finance, Baruch College, City University of New York, August 1987.

Linda C. McCarthy, Centre College: assistant professor, Union College, September 1, 1987.

Therese McCarty, University of Michigan: assistant professor, Union College, September 1987.

Robert H. McGuckin: chief, Center for Economic Studies, Bureau of the Census, December 1986.

Walter J. Mayer: assistant professor of economics, department of economics, University of Mississippi, August 11, 1987.

Harriet B. Newburger, University of Vermont: assistant professor, Bryn Mawr College, September 1, 1987.

Thomas I. Palley, Wesleyan University: assistant professor of economics, Amherst College, July 1, 1987.

V. Kerry Smith, Vanderbilt University: professor, department of economics and business, North Carolina State University, August 1987.

Timothy E. Sullivan, Wellesley College: assistant professor of economics, Elon College, August 1987.

Chein-Hsing Sung, Wayne State University: assistant professor, division of social science, Northeast Missouri State University, January 1987.

Rogério Zandamela: assistant professor, department of economics, Eastern Michigan University, September 1, 1986.

Leaves for Special Appointments

Christopher Garbacz, University of Missouri-Rolla: visiting professor of economics, University of Auckland, May–September 1987.

David Schap, College of the Holy Cross: visiting scholar, Center for Study of Public Choice, September 1987–May 1988.

John J. Seater, North Carolina State University: visiting professor, Princeton University, August 16, 1987–May 16, 1988.

Resignation

Marsha J. Courchane, North Carolina State University, May 15, 1987.

James A. Johnson, Wilfrid Laurier University, June 30, 1987.

Dean A. Schiffman, University of North Carolina-Chapel Hill, July 1, 1987.

David D. Steiner, University of North Carolina-Chapel Hill, July 1, 1987.

◆

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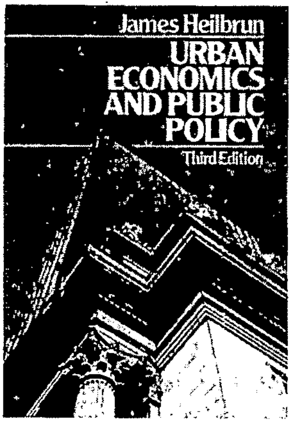
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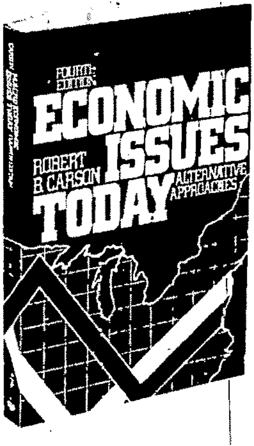
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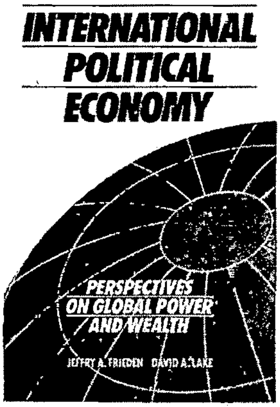
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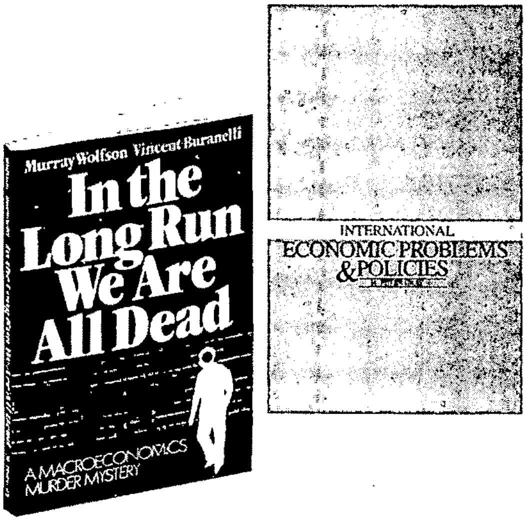
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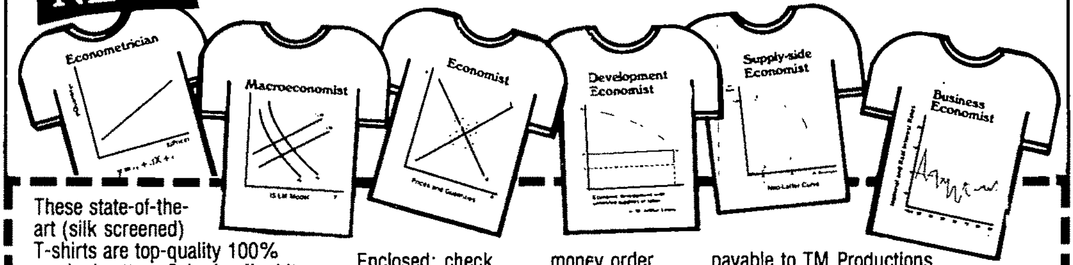
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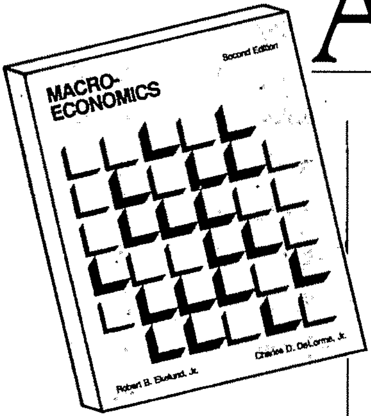
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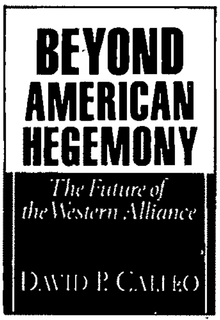

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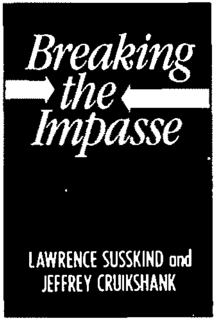

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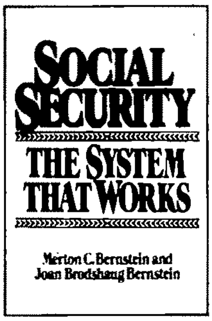

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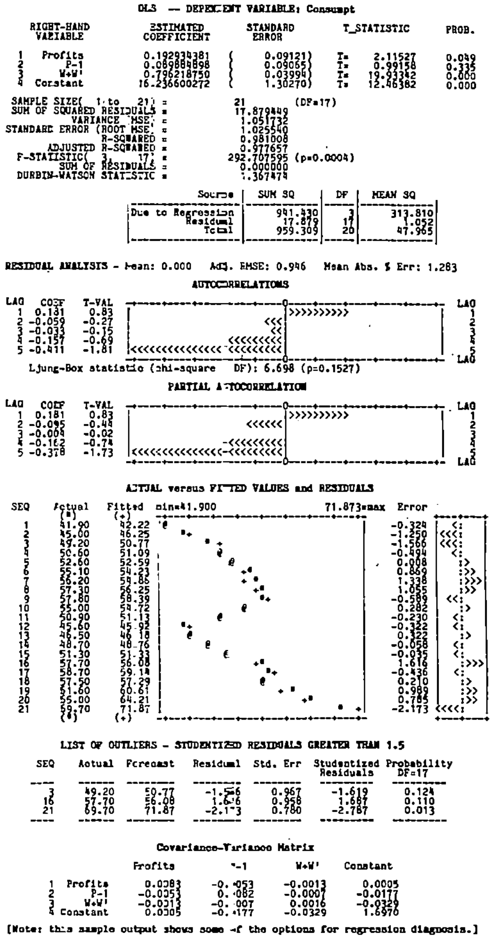

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 3       W+W'         0.796218750   (    0.03994)   T=    19.93342        0.000
 4   Constant        16.236600272   (    1.30270)   T=    12.46382        0.000

  SAMPLE SIZE(   1 to   21) =          21         (DF=17)
  SUM OF SQUARED RESIDUALS =           17.879449
           VARIANCE (MSE) =            1.051732
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                R-SQUARED =            0.981008
       ADJUSTED R-SQUARED =            0.977657
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                              Source    |   SUM SQ   |  DF  |   MEAN SQ
                    --------------------|------------|------|------------
                    |Due to Regression  |    941.430 |     3|     313.810|
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                    |            Total  |    959.309 |    20|      47.965|
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RESIDUAL ANALYSIS - Mean: 0.000   Adj. RMSE: 0.946   Mean Abs. % Err: 1.283

                                AUTOCORRELATIONS

LAG   COEF    T-VAL   -+----+----+----+----+----0----+----+----+----+----+-   LAG
 1  0.131     0.83    |                          |>>>>>>>>>>              |    1
 2 -0.059    -0.27    |                       <<<|                        |    2
 3 -0.033    -0.15    |                        <<|                        |    3
 4 -0.157    -0.69    |                 <<<<<<<<<|                        |    4
 5 -0.411    -1.81    |<<<<<<<<<<<<<<<<-<<<<<<<<<|                        |    5
                      -+----+----+----+----+----0----+----+----+----+----+-   LAG
   Ljung-Box statistic (chi-square   DF): 6.698 (p=0.1527)

                             PARTIAL AUTOCORRELATION

LAG   COEF    T-VAL   -+----+----+----+----+----0----+----+----+----+----+-   LAG
 1  0.181     0.83    |                          |>>>>>>>>>>              |    1
 2 -0.095    -0.44    |                    <<<<<<|                        |    2
 3 -0.004    -0.02    |                          |                        |    3
 4 -0.162    -0.74    |                -<<<<<<<<<|                        |    4
 5 -0.378    -1.73    |<<<<<<<<<<<<<<<<-<<<<<<<<<|                        |    5
                      -+----+----+----+----+----0----+----+----+----+----+-   LAG

                    ACTUAL versus FITTED VALUES and RESIDUALS

SEQ    Actual    Fitted  min=41.900                   71.873=max  Error
         (*)       (+)   -+----+----+----+----+----+----+----+--+          +----+----+
  1     41.90     42.22  '@                                   |   -0.324  |   <:    |
  2     45.00     46.25      *+                               |   -1.250  |<<<:     |
  3     49.20     50.77          *  +                         |   -1.566  |<<<:     |
  4     50.60     51.09 |           @                         |   -0.494  |   <:    |
  5     52.60     52.59 |             @                       |    0.008  |    :>   |
  6     55.10     54.23 |                +*                   |    0.869  |    :>>  |
  7     56.20     55.86 |                +  *                 |    1.338  |    :>>> |
  8     57.30     56.25 |                  +*                 |    1.055  |    :>>  |
  9     57.80     58.39 |                   *+                |   -0.589  |  <<:    |
 10     55.00     54.72 |               @                     |    0.282  |    :>   |
 11     50.90     51.13 |           @                         |   -0.230  |   <:    |
 12     45.60     45.92 |    *+                               |   -0.322  |   <:    |
 13     46.50     46.18 |     @                               |    0.322  |    :>   |
 14     48.70     48.76 |        @                            |   -0.058  |   <:    |
 15     51.30     51.33 |           @                         |   -0.035  |   <:    |
 16     57.70     56.08 |                  +*                 |    1.616  |    :>>> |
 17     58.70     59.14 |                    *+               |   -0.436  |   <:    |
 18     57.50     57.29 |                   @                 |    0.210  |    :>   |
 19     61.60     60.61 |                     +  *            |    0.989  |    :>>  |
 20     65.00     64.21 |                         +*          |    0.785  |    :>>  |
 21     69.70     71.87 |                              *   +  |   -2.173  |<<<<:    |
         (*)       (+)   -+----+----+----+----+----+----+----+--+          +----+----+

          LIST OF OUTLIERS - STUDENTIZED RESIDUALS GREATER THAN 1.5

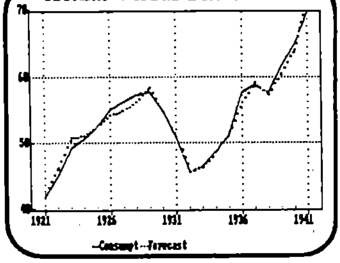
  SEQ     Actual   Forecast   Residual   Std. Err  Studentized Probability
                                                     Residuals     DF=17
  ----    ------   --------   --------   --------  ----------- -----------
     3     49.20      50.77     -1.566      0.967      -1.619       0.124
    16     57.70      56.08      1.616      0.958       1.687       0.110
    21     69.70      71.87     -2.173      0.780      -2.787       0.013
  ----    ------   --------   --------   --------  ----------- -----------

                        Covariance-Variance Matrix

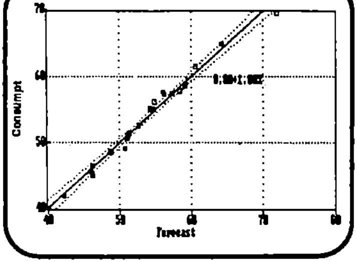
                   Profits          P-1         W+W'     Constant

    1  Profits      0.0083      -0.0053      -0.0013       0.0005
    2      P-1     -0.0053       0.0082      -0.0007      -0.0177
    3     W+W'     -0.0013      -0.0007       0.0016      -0.0329
    4 Constant      0.0005      -0.0177      -0.0329       1.6970

 [Note: this sample output shows some of the options for regression diagnosis.]
```



Actual Versus Fitted



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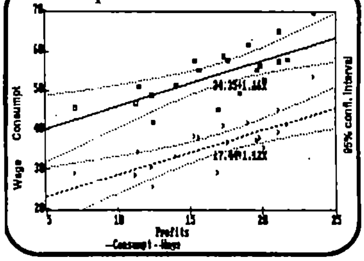

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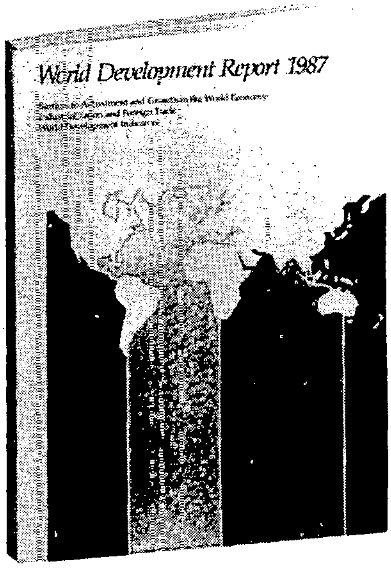

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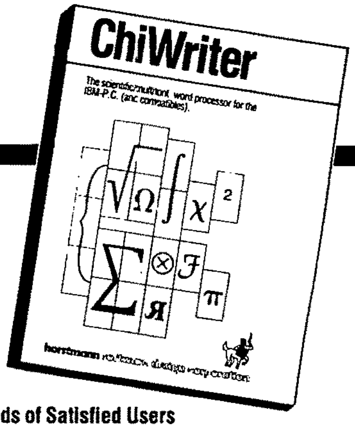
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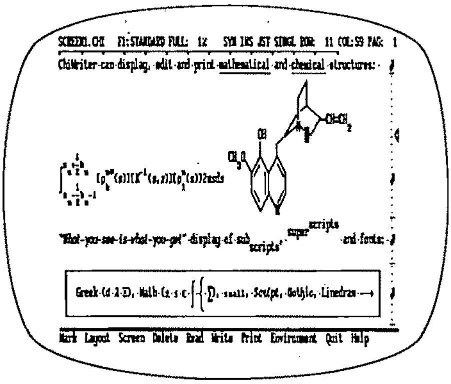
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